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ON HEREDITY AND REJUVENATION.[1]

BY CHARLES SEDGWICK MINOT.[2]

The subject of this article is presented under the following sections:

I. The Formative Force of Organisms.
II. The Conception of Death.
III. A Comparison of Larva and Embryo.
IV. Concluding Remarks.

The first section is not new, but a reproduction without change, of an article published in *Science*, July 3d, 1885. As this article has not become generally known, and yet is an essential link in the chain of reasoning, I venture to repeat it. Though written in 1885, I consider that to-day it is still sufficient to disprove Weismann's theory of germ plasm. Weismann has not considered this article, otherwise, from my point of view, he could not have maintained his theory.

[1] This article is translated from one which appeared in the Biologisches Centralblatt, Vol. XV, Page 571, August 1st, 1895. A few trifling changes have been made in the text. An abstract of the article was read before the American Association for the Advancement of Science, at its recent Springfield meeting.

[2] Professor in the Harvard Medical School.

The views which I then defended have been recently brought forward in almost parallel form, and without essential additions, by O. Hertwig (*Zeit-und Streitfragen der Biologie*, I, Heft, D. 32–53) as arguments against the views of Weismann.

The second section is also directed against Weismann, for it attempts to replace his conception of death by one more exact.

The third section is intended to make the significance of rejuvenation clear, and at the same time, by a comparison of larvæ and embryos, to demonstrate a law of heredity which has not been hitherto recognized.

The Formative Force of Organisms.

The assertion is safe, that the majority of biologists incline at present to explain the forming of an organism out of its germ upon mechanical principles. The prevalent conception is that the forces of the ovum are so disposed that the evolution of the adult organism is the mechanical result of the predetermined interplay of those forces. The object of the present article is to point out that this conception is inadequate, and must be at least supplemented, if not replaced, by another view, namely, that the formative force is a generally diffused tendency, so that all parts inherently tend to complete by their own growth and modification the whole organism—a fact which finds a legitimate hypothetical expression in Darwin's Doctrine of pangenesis. The nature of the view here advanced will become clearer upon consideration of the evidence upon which it is based, and which is adduced below. The evidence that the formative force is diffused through all parts falls under three heads: 1. The process of regeneration in unicellular and multicellular bionts; 2. The phenomena of of the duplication of parts; 3. All forms of organic reproduction. Let us briefly consider these categories.

1. *Regeneration.*—All living organisms have, to a greater or less degree, the ability to repair injuries; indeed, we must regard the power of regeneration as coextensive with life, but

the capacity varies enormously in the different species. In man the power is very small, though more extensive than is generally realized. Among Annelids are species, the individuals of which may be divided in two, and each piece can regenerate all that is needed to render it a complete worm. We sometimes see a small fragment of a plant, a single switch of a willow, for instance, regenerate an entire tree, roots, trunk, branches, leaves, flowers, and all. In the last instance a few cells possess a latent formative force, which we recognize by its effects, but cannot explain. We perceive, therefore, that each individual has, as it were, a scheme or plan of its organization to which it strives to conform. As long as it actually does so, the cells perform their routine functions; but when an injury destroys or removes some portion, then the remaining cells strive to conform again to the complete scheme, and to add the missing fragment. The act of regeneration of lost parts strikes the imagination almost as an intelligent pursuit by the tissues of an ideal purpose.

Our knowledge of the regeneration power has recently received important extensions through the noteworthy experiments of Nussbaum[3] and Gruber,[4] who have demonstrated, independently, the possibility of dividing unicellular animals so that each piece will regenerate the missing parts. In this manner the number of individuals can be artifically multiplied. For example: Nussbaum divided a well-isolated Oxytricha into two equal parts, either transversely or longitudinally, and found that the edges of the cut became soon surrounded with new cilia. Although some of the substance of the body, or even a nucleus, was lost through the operation yet, by the following day, the two parts converted themselves into complete animals with four nuclei and nucleoli (*Nebenkerne*) and the characteristic ciliary apparatus. "The head piece has formed a new hind end; the right half, a new left half." The

[3] M. Nussbaum, *Ueber spontane und kunstliche Zellteilung*, Sitzungsb. d. neiderrh. Ges. f. Nat. u. Heilkunde, Bonn, 15, Dez., 1884.

[4] A. Gruber, *Ueber kunstliche Teilung bei Infusorien*, Biol. Centralblatt, Bd. IV, No. 23, 717--722.

newformed duplicate Infusoria multiplied subsequently by spontaneous division. From one Oxytrachia cut in two, Nussbaum succeeded in raising ten normal animalcules, which subsequently all encysted. After an unequal division, the parts are both still capable of regeneration, but parts without a nucleus did not survive, which suggests that the formative energy is in some way bound up with the nucleus. But nucleate pieces may break down. Thus, all attempts at artificial multiplication of the multinucleate Opalina failed, although the division of Actinosphærium had been successfully made by Eichhorn as long ago as in the last century. Pelomyxa palustris has been successfully divided by Greef, and Myastrum radians by Haeckel.

Gruber (*l. c.*, p. 718) describes his experiments with Stentor: "If one divides a Stentor transversely through the middle, and isolates the two parts, one finds on the cut surface of the hind part, after about twelve hours, a complete peristomial field with the large cilia and buccal spiral newly formed. On the other hand, the piece on which the old mouth is situated has elongated itself backwards, and attached itself in the manner peculiar to these Infusoria. If one has made a longitudinal section, so that the peristom is cut in two, then the peristoms both complete themselves and the lateral wounds heal over. I have repeatedly separated, by transection, pieces considerably less than half of the original Stentor, and these have also regenerated themselves to complete animals." Gruber, too, observed that artificially divided Infusoria were capable of subsequent spontaneous multiplication. If the section is not very deep, there may arise double monsters; but here, just as in spontaneous divisions, as long as there remains an organic connecting band, the two parts act as one individual, showing that the nervous actions are not restricted to determined paths. Gruber also adds that two divided pieces may be reunited if brought together quickly enough. The observation thus briefly announced is of such extreme interest and importance that the publication of the full details of the experiment will be eagerly awaited. Gruber adds that at present we can-

not go much beyond the proof of existence, to a high degree, of the regenerative capacity in unicellular organisms, He also makes the significant observation that in the Protozoa, we have to do foremost with changes of function; in the Metazoa, with growth also.

2. *Duplication of parts.*—In these anomalies we find an organ which, although an extra member, yet still conforms to the type of the species. For example: a frog is found with three posterior limbs; dissection proves the third leg to agree anatomically with the typical organization of the frog's hind leg. In determining the importance to be attributed to this evidence, it should be remembered, on the one hand, that these instances are by no means unusual; on the other, that the agreement with the normal structure is not uniform.

3. *Asexual reproduction.*—When a species multiplies by fission of any kind, we must assume that each part, after division, possesses the formative tendency, since we see it build up what is necessary so complete the typical organization of the individual. Again: a bud of a hydroid or polyzoon, although comprising only a small part of the body, is equally endowed with this uncomprehended faculty. In pseudova we reach the extreme limit; in aphis, for example, the parent gives off a single cell, the capacity of which, to produce a perfect and complicated individual, fully equals the like capacity of a hydroid bud or of half a worm.

The evidence forces us to the conclusion that the formative force or cause is not merely the original disposition of the forces and substances of the ovum, but that *to each portion of the organism is given:* 1. *The pattern of the whole organism;* 2. *The partial or complete power to reproduce the pattern.* The italicized formula is, of course, a very crude scientific statement, but it is the best which has occurred to me. The formative force, then, is a diffused tendency. The very vagueness of the expression serves to emphasize our ignorance concerning the real nature of the force. In this connection, I venture to insist upon the fact that we know little or nothing concerning any of the fundamental properties of life, because I think the

lesson of our ignorance has not been learned by biologists. We encounter, not infrequently, the assertion that life is nothing but a series of physical phenomena; or, on the other hand, what is less fashionable science just now, that life is due to a special vital force. Such assertions are thoroughly unscientific; most of them are entirely, the remainder nearly worthless. Of what seems to me the prerequisites to be fulfilled before a general theory of life is advanced, I have written elsewhere.[5]

II. Conception of Death.

My thesis reads: There are two forms of death. These are *first*, the death of the single cells; *second*, the death of multicellular organisms. Death in the one case is not homologous with death in the other.

Weismann assumed the complete homology of the two forms of death. Without this assumption, his hypothesis of the immortality of unicellular organisms falls to the ground and with it falls the entire superstructure of his speculations upon germ plasm. Oscar Hertwig (*Zeit und Streitfragen, Heft* 1) has already expounded, very clearly, the dependence of the theory of germ plasm upon the hypothesis of unicellular immortality; it would, therefore, be superfluous to discuss it here.

The conception of the biological problem of death, to which I still hold, was formed several years before Weismann's first publication, which appeared in 1882, with the title, "*Ueber die Dauer des Lebens.*" He has further defended his view in his article, "*Ueber Leben und Tod*" (1884), and has steadfastly adhered to it since. In the years 1877–1879 I published my theoretical interpretation of the problem.[6] This interpretation became the starting point of elaborate special investigations, by which I endeavored to advance the solution of the problem and, in fact, observation and experiment have confirmed the

[5] C. S. Minot, *On the conditions to be filled by a theory of life*, Proc Amer. Assoc. Adv. Sc., XXVIII, 411.

[6] Proc. Boston Soc. Nat. Hist., XIX, 167; XX, 190.

original thesis.[7] Moreover, in an especial short article I have directed attention to the fact that Weismann has not considered the essential issue of the problem. The difficulties pointed out still remain, and, according to my conviction, cannot be removed. Weismann passes these difficulties by and carries out his speculations without first securing a basis for them. His method is illustrated by the following quotation: "I have, perhaps, not to regret that I cannot here discuss the article referred to (Minot's Article in *Science*, Vol. IV, p. 398); nevertheless, almost all objections which are there made to my views are answered in the present paper." (Weismann, Zur Frage nach der Unsterblichkeit der Einzelligen, Biol. Centralbl., IV, 690, Nachschrift). I have studied the paper with conscientious care and cannot admit that the objections have been answered. On the contrary, I maintain now, as formerly, the judgment: "He misses the real problem." For this reason I hold it to be unnecessary to discuss the details of Weisman's exposition, because—if I am right—he has not considered the actual problem of death at all. "He misses the real problem." The following reasoning leads to this decision: Protozoa and Metazoa consist of successive generations of cells; in the former the cells separate; in the latter they remain united; the death of a Protozoa is the annihilation of a cell, but the death of a Metazoon is the dissolution of the union of cells. Such a dissolution is the result of time, that is to say, of the period necessary to the natural duration of life, and we call it, therefore, "*natural death.*" Moreover, we know that natural death is brought about by gradual changes in the cells until, at last, certain cells, which are essential to the preservation of the whole, cease their functions. Death, therefore, is a consequence of changes which progress slowly through successive generations of cells. These changes cause senescence, the end of which is given by death. If we wish to know whether death, in the sense of natural death, properly so called, occurs in Protozoa or not, we must first pos-

[7] Journal of Physiology, XII, and Proc. A. A. A. S., XXXIX, (1890).

sess some mark or sign, by which we can determine the occurrence or absence of senescence in unicellular organisms.

Around this point the whole discussion revolves. Certainly a simpler and more certain conclusion could hardly be drawn than that the death of a Metazoon is not identical, *i. e.*, homologous with the death of a single cell. Weismann tacitly assumed precisely this homology, and bases his whole argument on it. In all his writings upon this subject, he regards the death of a Protozoon as immediately comparable with the death of a Metazoon. If we seek from Weismann for the foundation of this view we shall have only our labor for our pains. Starting from this view Weismann comes to the strictly logical conclusion that the Protozoa are immortal. This is a paradox! In fact, if one compares death in the two cases, from Weismann's standpoint, then we must assume a difference in the causes of death, and conclude that the cause in the case of the Protozoa is external only, while in the Metazoa it is internal only, for, of course, we may leave out of account the accidental deaths of Metazoa. If we approach the problem from this side, we encounter the following principal question: Does death from inner causes occur in Protozoa? Weismann gives a negative answer to this question, with his assertion that unicellular organisms are immortal. The assertion remains, but the proof of the assertion is lacking. In order to justify the assertion, it must be demonstrated that there does not occur in Protozoa a true senescence, showing itself gradually through successive generations of cells. Has Weismann furnished this demonstration? Certainly not. He has, strictly speaking, not discussed the subject. It is clear that we must first determine whether natural death from senesence occurs in Protozoa or not, before we can pass to a scientific discussion of the asserted immortality of unicellular beings. The problem cannot be otherwise apprehended. Weismann has not thus conceived it, therefore the judgment stands against him: *he misses the real problem.*

Senesence has been hitherto little investigated; for many years I have been studying it experimentally and have tried

to determine its exact course. My paper, "Senesence and Rejuvenation," affords evidence of new facts proven by these experiments. I believe I have thus won the right to oppose my view to the pure speculations of Weismann.

(*To be continued.*)

LOST CHARACTERISTICS.

By Alpheus Hyatt.

Dr. Minot having noticed, in the translation of his article "On Heredity and Rejuvenation," an accidental omission of quotation of work done by paleontologists on the loss of characteristics in the development of animals, has most courteously asked me to follow his essay by an article dealing with this question. I gladly avail myself of this opportunity on account of the advantages offered where similar subjects can be consecutively treated from different points of view, and because Dr. Minot's article, on account of his great and deserved reputation in embryology, will reach the students of existing biological phenomena, and perhaps induce some of them to read a connected publication.

The loss of characteristics is not so readily observed by a student of the biology of existing animals or neobiologist, as by the paleobiologist or student of fossils, because the latter necessarily deals with series of forms often persisting through long periods of time, and is led, especially if he follow more recent methods of research, to study these in great detail. The observer of these remains is not, as is falsely imagined, limited to fragments, but can and does work out of the hard matrix the external skeletons or shells even of embryos, and can, in the corals, brachiopoda, mollusca, echinodermata and even in protozoa, follow the entire life history of these parts in the individual. He has also the further advantage of availing himself of the knowledge amassed by the neobiologist and neoembryologist, the works of Cope, Beecher, Schuchert, Gurley,

Jackson and others, written in the last thirty years in this country and in Europe. The new school of Paleobiology also insists upon the close study of series of forms and rejects the methods usually pursued by the neoembryologist, who, as a rule, selects his objects of study and pursues his comparisons upon the old basis of comparative anatomy and with but little regard to the serial connections of forms. The importance of studying the seriality in structure of the members of the same group, those gradations, which lead from one variety to another, one species to another, one genus to another, until they may end in highly differentiated and often degraded offshoots, with as strange and unique developments as they have adult characters, seems not, as yet, to have attracted the attention of the students of development among recent animals as it has that of paleobiologists. The prevalent modes of study of living types has consequently led to noticing the phenomena of omission of hereditary characters only in an isolated way, and from the time of Balfour's "Comparative Embryology" these omissions occurring in the embryo have been named abbreviations, shortenings and omissions of development, and various attempts have been made to explain them upon more or less general grounds of inference. Prof. Cope and the writer and some other authors have been for a number of years publishing observations upon this class of phenomena under the title of the law of acceleration, asserting that in following out the history of series in time, or of existing series in structure, there was observable a constant tendency in the successive members (species, genera, etc.) of the same natural group to inherit the characters of their ancestors at earlier stages than those in which they appeared in these ancestors. That as a corollary of this tendency, the terminal forms eventually skipped or omitted certain ancestral characteristics, which were present in the young of the preceding or normal forms of the same series. and also in the adult stages of development of more remote ancestors of the same genetic stock or series. This law has since been independently rediscovered by several other naturalists, notably Würtemburger in Germany, and Buckman in England. The writer has lately christened this as the law of

Tachygenesis[1] in allusion to the general character of the phenomena.

In a late paper,[2] the writer reviewed Prof. Cope's and Haeckel's views of this law, and contrasted them with his own, and it seems advisable to give these remarks again in this connection.

Professor Cope has given the fullest explanation of this law, but has joined it with retardation. Thus, from his point of view, if I rightly understand him, inexact parallelism in development or failure to reproduce any hereditary characteristics is due to a tendency which appears in organisms and works in parallel lines with acceleration, the law being in his conception of a double nature. Thus he says, on page 142 of his "Origin of the Fittest," "The acceleration in the assumption of a character progressing more rapidly than the same in another character, must soon produce, in a type whose stages were once the exact parallel of a permanent lower form, the condition of inexact parallelism. As all the more comprehensive groups present this relation to each other, we are compelled to believe that acceleration has been the principle of their successive evolution during the long ages of geologic time. Each type has, however, its day of supremacy and perfection of organism, and a retrogression in these respects has succeeded. This has, no doubt, followed a law the reverse of acceleration, which has been called retardation. By the increasing slowness of the growth of the individuals of a genus, and later and later assumption of the characters of the latter, they would be successively lost. To what power shall we ascribe this acceleration by which the first beginnings of structure have accumulated to themselves through the long geologic ages, complication and power, till from the germ that was scarcely born into a sand lance, a human being climbed the complete scale, and stood easily the chief of the whole." And again, on page 182 of the same work : "Acceleration signifies addition to the number of those repetitions during the period

[1] "Phylogeny of an Acquired Characteristic." Proc. Am. Phil. Soc. Philadelphia, XXXII, No. 143.

[2] "Bioplastology and the Related Branches of Biologic Research." Proc. Bost. Soc. Nat. Hist., XXVI, p. 77–81.

preceding maturity, as compared with the preceding generation, and retardation signifies a reduction of the numbers of such repetitions during the same time." Thus, from Cope's point of view, tachygenesis is the law of progression, and retardation is the law of retrogression, and they are both essential parts of his law of acceleration and retardation.

Haeckel alludes in general terms to the law of abbreviated development in his "Morphologie der organismen," and in his "Anthropogenie," published in 1874, substantially agrees with Cope in his view of the law and uses the term "palingenesis" for the exact repetition of characteristics which occurs in the earlier and simpler forms of a phylum and "coenogenesis" for the abbreviated or highly accelerated cases of inexact parellism of the young of more complex forms with their ancestors. There is, however, an objection to this mode of using the last term which I mentioned also in writing the paper quoted.[3]

[3] During the writing of this paper I took from Cope the statement made above, although unable to find any verification of it in Haeckel's Anthropogenie (1st and 2d editions both dated 1874), but, since the above was in press, I obtained a copy of the 4th edition (1891) and the reading of this has caused me to entirely alter my opinion with regard to Haeckel's opinions. He certainly had at that time, 1891, what seems to me erroneous and inadequate view of the nature and action of the laws of tachygenesis and gave it too limited application. He also used the terms, palingenesis and cenogenesis differently from the way in which Cope and others have used them in this country.

Haeckel states (Anthropogenie, 4th edition, Leipzig, p. 9, 1891) that "Palingenetische Processe oder keimesgeschichtliche Wiederholungen nennen wir alle jene Erscheinungen in der individuellen Entwickelungsgeschichte, welche durch die conservative Vererbung getreu von Generation zu Generation übertragen worden sind und welche demnach einen unmittelbared Rückschluss auf entsprechende Vorgänge in der stammesgeschichte der entwickelten Vorfahren gestatten. Cenogenetische Processe hingegen oder keimesgeschichtlliche *Störungen* nennen wir alle jene vorgänge in der keimesgeschichte, *whelche* nicht auf solche vererbung von uralten stammformen zurüchführbar, vielmehr *erst spater durch Anpassung der Keime oder der Jugendform* an bestimme Bedingungen der Keimesentwickelung hinzugekomen sind. Diese ontogenetischen Erscheinungen *sind fremde zuthaten* welche *durchaus keinen unmittelbaren Schluss anf entsprechende Vorgange in der stammesgeschichte* der Ahnenreihe erlauben, vielmehr die Erkenntniss der letzteren geradezu fälschen und verdecken."

So far as one can get at Haeckel's oplnions from such expressions as the above it is obvious that he views shortened or abbreviated development in a very distinct light from that to which I am accustomed. He speaks of it as due to the introduction of "fremde zuthaten" as "Cenogenetische oder Störungsgeschichte"

and further to make his meaning clearer, on page 11 he divides cenogenetic phenomena into " Ortsverschiebungen oder Heterotopien," and, on page 12, "Zeitverschiebungen oder Heterochronien." Organs or parts may be developed heterotopically, that is, out of place or in a different part of the body from that in which they originated in the ancestors; or heterochronically, that is earlier in time during the life of the individual than that in which they originated, and he also speaks of the latter as "ontogenetische Acceleration," using exactly the adjective applied in this country many years beforehand, but that fact does not seem to have been considered worthy of his attention. Haeckel then proceeds to add: " Das umgekehrte gilt von der verspäteten Ausbildung des Darmcanals, der Leibeshöhle, der Geschlechtsorgane. Hier liegt offenbar eine Verzögerung oder Verspätung, eine *ontogenetische Retardation*." This is probably what Cope alludes to in his quotation of Haeckel, and certainly this is a restatement of Cope's law of retardation with, however, the ommission of any reference to the original discoverer. It will be gathered from the text above that I view acceleration firstly, as a normal mode of action or tendency of heredity acting upon all characters that are genetic, or, in other words, derived from ancestral sources; secondly, that a ctetic, or, in other words, a newly acquired character must become genetic before it becomes subject to the law of tachygenesis. Haeckel has evidently confused ctetic characters like those of the so called ovum of Taenia, the Pluteus of Echinoderms and the grub, maggot, caterpillars of insects, which have caused the young to deviate more or less from the normal line of development, as determined by the more generalized development of allied types of the same divisions of the animal kingdom, with the normal characters that are inherited at an early stage in the ontogeny and considers them all as heterochronic. It is very obvious that they are quite distinct and that, while the ctetic characters may have been larval or even possibly embryonic in origin, and may not have affected perceptibly the adult stage at any time in the phylogeny of the group, they are, nevertheless, subject to the law of acceleration and do affect the earliest stages as has been shown in Hyatt's and Arm's book on Insecta. Such characteristics do, of course, contradict the record, if we consider that the record ought have been made by nature according to anthropomorphic standards, and in such misleading phraseology they are falsifications of the ontogenetic recapitulation of the phylogeny. In a proper nomenclature, framed with due regard to natural standards, such expressions are inadmissable. There is absolutely no evidence that characteristics repeated in the younger stages of successive species and types owe their likeness to ancestral characters to other causes than heredity. This likeness may be interfered with or temporarily destroyed by extraordinary changes of habit, as among the larvae of some insecta and the forms alluded to above, or among parasites in different degrees, but the obvious gradations of structures in many of these series show that hereditary tendencies are not easily changed in this way. There are comparatively very few forms having doubtful affinities even among the parasites. It is also evident that the novel larval characters originating in the young in their turn speedily become hereditary and are incorporated in the phylogeny and recapitulated in the ontogeny.

It may be seen from this that in dividing tachygenesis into palingenesis and cenogenesis the writer has followed Cope rather than Haeckel, and there is a seri-

Either through want of acquaintance with good examples of retardation or because of a different point of view, I have not been able to see any duplex action in the law of acceleration. To me it is the same law of quicker inheritance which is acting all the time in the phylum at the beginning, middle, and end of its history, as will be seen by the explanation given above. In Insecta[4] I have tried to apply it to the explanation of the peculiar larval forms of those animals which often present retrogression through suppression of ancestral characters in the young, although their adults are perfectly normal and perhaps progressive. Consequently, palingenesis and coengenesis are, from my point of view, simply different forms of tachygenesis, and there is no boundary or distinction between them. In other words, retardation or retrogression occurs because of the direct action of tachygenesis upon more suitable and more recently acquired characteristics which are driven back upon and may directly replace certain of the ancestral characters causing them to disappear from ontogenetic development.[5]

ous objection to the use of cenogenesis at all, since it is from *Κενός* meaning strange, and was first applied by Haeckel in such a way that both by his statements, and the derivation, it ought to be confined to types like larvae of the Echinodemata Insect, etc., and parasites in which acquired characters do interfere with the ontogenic recapitulation for a certain time. Normal types, in which tachygenesis occur in a marked way might be called tachygenetic. Palingenesis and palingenetic might be confined to generalized forms in which the ontogeny was a more or less prolonged recapitulation of the phylogeny, and coenogenesis would thus be properly confined to its original field wherever ctetic characters were introduced. This would avoid the need of using a new term.

[4] Guides for Science Teaching, Boston Soc. Nat. Hist., No. 8.

[5] Specialization by reduction of parts is evidently included under the head of retardation by Cope; thus in Origin of the Fittest, p. 353, he says that "change of structure during growth is accomplished either by addition or parts (acceleration) or by subtraction of parts (retardation)." So far as my experience goes in the major number of cases, the parts of characters that are undergoing reduction disappear according to the law of tachygenesis. They reappear in the ontogeny at earlier and earlier stages, or exhibit this tendency in the same way as characters of the progressive class, but their development is not so complete as in ancestral forms. In this sense they can be regarded as retarded or thrown back in their development. There is, however, another way of formulating the expression for this. Instead of regarding this disappearance by retrogressive gradations as due to a tendency opposed to acceleration, is it not a tendency of the same

The law of tachygenesis as defined by the writer acts upon all characteristics and tendencies alike, and is manifested in genetically connected phyla by an increasing tendency to concentrate the characteristics of lower, simpler, or earlier occurring, genetically connected forms in the younger stages of the higher, more complicated or more specialized, or more degraded, or later occurring forms of every grade, whether the characteristics arise in adults or in the younger stages of growth. Since my first publication in 1866, the law has become clearer to me, but I have made no fundamental change in the conception. The application of the law to degenerative characteristics appears to me to explain why there are degenerative forms in the phylum which are indicated by the senile stages of the individual.

The degenerative changes of the senile period may, and practically in all cases do, tend to the loss of characteristics of the adult period and consequently in extreme cases bring about not only the loss of a large proportion of progressive characteristics, but loss in actual bulk of the body as compared with adults, as has been stated above. This is usually regarded as due to the failure of the digestive organs or defective nutrition, and this may be true in many examples; but, on the other hand, it often begins in individuals long before there is any perceptible diminution in size, and may occur in dwarfs and in some degenerate species in the early stages, and finally in series of species according to the law of tachygenesis, so that

kind? That is to say, do not the parts and characters show a tendency to disappear earlier and earlier, and are they not, in most cases, at the time of disappearance, present also in earlier stages of growth than that in which they originated in ancestral forms?

Is not the case of the wisdom teeth exceptional? The frequently extremely late external appearance of these is not accompanied by a later origin of their rudiments in the jaw. Although they may not appear in many cases above the gum until a person is past fifty, is not this retardation in becoming externally visible due primarily to the fact that they are deficient in growth power (tending to disappear from disuse, etc.), and secondarily to their internal position. When they cease to be able to break through the gum, will they not still continue to develop at the same stage as the other teeth, and will not their rudiments be likely tobe present at this early stage long after they have ceased developing into perfect teeth?

one is led to believe that the tendency to the earlier inheritence of degenerative modifications producing retrogression is inheritable like the tendency to the earlier inheritance of additional or novel characteristics producing progression. Thus, this law applied to progressive or retrogressive groups explains the mode in which their progression or retrogression is accomplished so far as the action of the laws of genesiology (science of heredity) are concerned.

In the same essay on Bioplastology, the writer reviewed Dr. Minot's law of growth, and in this and in his Phylogeny, quoted above, used it to throw light upon one of the most difficult problems of evolution.

It is a general law of unique importance, as readily observable in the growth of skeletons and shells of all kinds, and therefore as obvious in fossils as in the famous guinea pigs studied by Dr. Minot. This law enabled the writer to get what seemed to him a clearer view of the action of tachygenesis. See Bioplastology (p. 76).

Minot's researches enable one to see clearly that the reduction of parts or characteristics which takes place through the action of the law known as the law of acceleration in development (often also descriptively mentioned as abbreviated or concentrated development) cannot be considered as due to growth.

" It seems probable from my own researches published in various communications, but more especially in the ' Genesis of the Arietidae,'[6] that the action in this case is *a mechanical replacement of the earlier and less useful ancestral characteristics and even parts by those that have arisen later in the history of the group.* We can fully understand the phenomena of acceleration in development only when we begin by assuming that the characteristics last introduced in the history of any type were more suitable to the new conditions of life on the horizon of occurrence of the species than those which characterized the same stock when living on preceding horizons or in less specialized habitats. These new characters would necessarily, on

[6] Smithsonian Contributions to Knowledge, v. 26, p. 40–48, 1889; also, Mus. Comp. Zool., v. 16, 1889.

account of their greater usefulness and superior adaptability, ultimately interfere with the development of the less useful ancestral stages and thus tend to replace them. The necessary corollary of this process would be tachygenesis or earlier appearrance of the ancestral stages in direct proportion to the number of new characteristics successively introduced into the direct line of modification during the evolution of a group.

If this be true, it can hardly be assumed that the loss of characteristics and parts taking place in this way is directly due to growth force. If growth has anything to do with these phenomena, it must act indirectly, and, as in the repetition of other similarities and parallelisms, under the controlling guiddance of heredity.

VARIATION AFTER BIRTH.

By L. H. Bailey.

At the present time, our attention is directed to differences or variations which are born with the individual. We are told that variation which is useful to the species is congenital, or born of the union—or the amalgamation in varying degrees—of parents which are unlike each other. From the variations which thus arise, natural selection chooses those which fit the conditions of life and destroys the remainder. That is, individuals are born unlike and unequal, and adaptation to environment is wholly the result of subsequent selection.

These are some of the practical conclusions of the NeoDarwinian philosophy. It seems to me that we are in danger of letting our speculations run away with us. Our philosophy should be tested now and then by direct observation and experiment, and thus be kept within the limits of probability. The writings of Darwin impress me in this quality more than in any other,—in the persistency and single-mindedness with which the author always goes to nature for his facts.

In this spirit, let us drop our speculations for a moment, and look at some of the commonest phenomena of plant life as they transpire all about us. We shall find that, for all we can see, most plants start equal, but eventually become unequal. It is undoubtedly true that every plant has individuality from the first, that is, that it differs in some minute degree from all other plants, the same as all animals possess differences of personality; but these inital individual differences are often entirely inadequate to account for the wide divergence which may occur between the members of any brood before they reach their maturity.

The greater number of plants, as I have said, start practically equal, but they soon become widely unlike. Now, everyone knows that these final unlikenesses are direct adaptations to the circumstances in which the plant lives. It is the effort to adapt itself to circumstances which gives rise to the variation. The whole structure of agriculture is built upon this fact. All the value of tillage, fertilizing and pruning lies in the modification which the plant is made to undergo. Observe, if you will, the wheat fields of any harvest time. Some fields are "uneven," as the farmers say; and you observe that this unevenness is plainly associated with the condition of the land. On dry knolls, the straw is short and the plant early; on moister and looser lands, the plant is tall, later, with long, well-filled heads; on very rich spots, the plants have had too much nitrogen and they grow too tall and "sappy," and the wheat "lodges" and does not fill. That is, the plants started equal, but they ended unequal. Another field of wheat may be very uniform throughout; it is said to be "a good stand," which only means, as you can observe for yourself, that the soil is uniform in quality and was equally well prepared in all parts. That is, the plants started equal, and they remained equal because the conditions were equal. Every crop that was ever grown in the soil enforces the same lessons. We know that variations in plants are very largely due to diverse conditions which arise after birth.

All these variations in land and other physical conditions are present in varying degrees in wild nature, and we know

that the same kind of adaptations to conditions are proceeding everywhere before our eyes. We cannot stroll afield without seeing it. Dandelions in the hollows, on the hillocks, in the roadside gravel, in the garden—they are all different dandelions, and we know that any one would have become the other if it had grown where the other does.

But aside from the differences arising directly from physical conditions of soil and temperature and moisture, and the like, there are differences in plants which are forced upon them by the struggle for life. We are apt to think that, as plants grow and crowd each other, the weaker ones die outright, because they were endowed with—that is, born with—different capabilities of withstanding the scuffle. As a matter of fact, however, the number of individuals in any area may remain the same or even increase, whilst, at the same time, every one of them is growing bigger. Early last summer I staked off an area of twenty inches square in a rich and weedy bit of land. When the first observations were made on the the 10th of July, the little plat had a population of 82 plants belonging to 10 species. Each plant was ambitious to fill the entire space, and yet it must compete with 81 other equally ambitious individuals. Yet, a month later, the number of plants had increased to 86, and late in September, when some of the plants had completed their growth and had died, there was still a population of 66. The censuses at the three dates were as follows :

	July 10.	Aug. 13.	Sept. 25.
Crab grass (Panicum sanguinale) .	22	20	15
Black Medick (Medicago lupulina) .	16	17	15
Purslane	14	15	12
White Clover	12	13	8
Red Clover	9	11	8
Red-root (Amarantus retroflexus) .	4	4	4
Ragweed (Ambrosia artemisiæfolia) .	2	2	2
Pigeon-grass (Setaria glauca) . .	1	2	3
Pigweed (Chenopodium album) .	1	1	0
Shepherd's Purse	1	1	1
	82	86	66

What a happy family this was! In all this jostle up to the middle of August, during which every plant had increased its

bulk from two to twenty times, only the crab grass—apparently the most tenacious of them all—had fallen off; and yet the area seemed to be full in the beginning! How then, if all had grown bigger, could there have been an increase in numbers, or even a maintenance of the original population? In two ways: first, the plants were of widely different species of unlike habits, so that one plant could grow in a place where its neighbor could not. Whilst the pigweed was growing tall, the medick was creeping beneath it. This is the law of divergence of character, so well formulated by Darwin. It is a principle of wide application in agriculture. The farmer "seeds" his wheat-field to clover when it is so full of wheat that no more wheat can grow there, he grows pumpkins in a cornfield which is full of corn, and he grows docks and sticktights in the thickest orchards. Plants have no doubt adapted themselves directly, in the battle of life, to each other's company.

The second and chief reason for the maintenance of this dense population, was the fact that each plant grew to a different shape and stature, and each one acquired a different longevity; that is, they had varied, because they had to vary in order to live. So that, whilst all seemed to have an equal chance early in July, there were in August two great branching red-roots, one lusty ragweed and 83 other plants of various degrees of littleness. The third census, taken September 25th, is very interesting, because it shows that some of the plants of each of the dominant species had died or matured, whilst others were still growing. That is, the plants which were forced to remain small also matured early and thereby, by virtue of their smallness, they had lessened, by several days, the risk of living, and they had thus gained some advantage over their larger and stronger companions, which were still in danger of being killed by frost or accident. When winter finally set in, the little plat seemed to have been inhabited only by three big red-roots and two small ones and by one ragweed. The remains of these six plants stood stiff and assertive in the winds; but if one looked closer he saw the remains of many lesser plants, each "yielding seed after his kind," each one, no

doubt, having impressed something of its stature and form upon its seeds for resurrection of similar qualities in the following year. All this variation must have been the result of struggle for existence, for it is not conceivable that in less than two square feet of soil there could have been other conditions sufficiently diverse to have caused such marked unlikenesses; and I shall allow the plat to remain without defilement that I may observe the conflict in the years to come, and I shall also sow seeds from some of the unlike plants. From all these facts, I am bound to think that physical environment and struggle for life are both powerful causes of variation in plants which are born equal.

Still,the reader may say,like Weismann,that thesedifferences were potentially present in the germ, that there was an inherited tendency for the given red-root to grow three feet tall when 85 other plants were grown alongside of it in twenty inches square of soil. Then let us try plants which had no germ plasm, that is, cuttings from maiden wood. A lot of cuttings were taken from one petunia plant, and these cuttings were grown singly in pots in perfectly uniform prepared soil, the pots being completely glazed with shellac and the bottoms closed to prevent drainage. Then each pot was given a weighed amount of different chemical fertilizer and supplied with perfectly like weighed quantities of water. All weak or unhealthy plants were thrown out, and a most painstaking effort was made to select perfectly equal plants. But very soon they were unequal. Those fed liberally on potash were short, those given nitrogen were tall and lusty; and the variations in floriferousness and maturity were remarkable. The data of maturity and productiveness were as follows:

Phosphate of Potash.	Sulphate of Potash.	Phosphate of Soda.	Check	Phosphate of Ammonia.
68 days	99 days	65 days	67 days	104 days
23½ blooms	18 blooms	27½ blooms	26½ blooms	33 blooms

Here then, is a variation of 39 days, or over a month in the time of first bloom, and of an average of 15 flowers per plant in asexual plants from the same stock, all of which started equal and which were grown in perfectly uniform conditions, save the one element of food.

But these or similar variations in cuttings are the commonest experiences of gardeners. Whilst some philosophers are contending that all variation comes through sexual union, the gardener has proof day by day that it is not so. In fact, he does not stop to consider the difference between seedlings and sexless plants in his efforts to improve a type, for he knows by experience that he is able to modify his plants in an equal degree, whatever the origin of the plants may have been. Very many of our best domestic plants are selections from plants which are always grown from cuttings or other asexual parts. A fruitgrower asked me to inspect a new blackberry which he had raised. "What is its parentage?" I asked. "Simply a selection from an extra good plant of Snyder" he answered; that is, selection by means of suckers, not by seedlings. The variety was clearly distinct from Snyder, whereupon I named it for him. The Snyder plants were originally all equal, all divisions in fact, of one plant, but because of change of soil or some other condition, some of the plants varied, and one of them, at least, is now the parent of a new variety.

But even Mr. Weismann would agree to all this, only he would add that these variations are of no use to the next generation, because he assumes that they cannot be perpetuated. Now, there are several ways of looking at this Weismannian philosophy. In the first place, so far as plants are concerned in it, it is mere assumption, and, therefore, does not demand refutation. In the second place, there is abundant asexual variation in flowering plants, as we have seen; and most fungi, which have run into numberless forms, are sexless. In the third place, since all agree that plants are intimately adapted to the conditions in which they live, it is violence to suppose that the very adaptations which are directly produced by those conditions are without permanent effect. In the fourth place, we know as a matter of common knowledge and also of direct experiment, that acquired characters in plants often are perpetuated.

I cannot hope to prove to the Weismannians that acquired characters may be hereditary, for their definition of an acquired

character has a habit of retreating into the germ where neither they nor anyone else can find it. But this proposition is easy enough of proof, viz., plants which start to all appearances perfectly equal, may be greatly modified by the conditions in which they grow; the seedlings of these plants may show these new features in few or many generations. Most of the new varieties of garden plants, of which about a thousand are introduced in North America each year, come about in just this way. A simple experiment made in our greenhouses also shows the truth of my proposition. Peas were grown under known conditions from seeds in the same manner as the petunias were, which I have mentioned. The plants varied widely. Seeds of these plants were saved and all sown in one soil, and the characters, somewhat diminished, appeared in the offspring. Seeds were again taken, and in the third generation the acquired characters were still discernible. The full details of this and similar experiments are waiting for separate publication. The whole philosophy of "selecting the best" for seed, by means of which all domestic plants have been so greatly ameliorated, rests upon the hereditability of these characters which arise after birth; and if the gardener did not possess this power of causing like plants to vary and then of perpetuating more or less completely the characters which he secures, he would at once quit the business because there would no longer be any reward for his efforts. Of course, the NeoDarwinians can say, upon the one hand, that all the variations which the gardener secures and keeps were potentially present in the germ, but they cannot prove it, neither can they make any gardener believe it; or, on the other hand, they can say that the new characters have somehow impressed themselves upon the germ, a proposition to which the gardener will not object because he does not care about the form of words so long as he is not disputed in the facts. Weismann admits that "climatic and other external influences" are capable of affecting the germ, or of producing "permanent variations," after they have operated "uniformly for a long period," or for more than one generation. Every annual plant dies at the end of the season, therefore whatever effect the environment may

have had upon it is lost, unless the effect is preserved in the seed; and it does not matter how many generations have lived under the given uniform environment, for the plant starts all over again, *de novo*, each year. Therefore, the environment must affect the annual plant in some one generation or not at all. It seems to me to be mere sophistry to say that in plants which start anew from seeds each year, the effect of environment is not felt until after a lapse of several generations, for if that were so the plant would simply take up life at the same place every year. This philosophy is equivalent to saying that characters which are acquired in any one generation are not hereditary until they have been transmitted at least once!

My contention then, is this: plants may start equal, either from seeds or asexual parts, but may end unequal; these inequalities or unlikenesses are largely the direct result of the conditions in which the plants grow; these unlikenesses may be transmitted either by seeds or buds. Or, to take a shorter phrase, congenital variations in plants may have received their initial impulse either in the preceding generation or in the sexual compact from which the plants sprung.

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A COMPARATIVE STUDY OF THE POINT OF ACUTE VISION IN THE VERTEBRATES.[1]

By J. R. Slonaker,
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In this preliminary sketch of a comparative study of the eyes of vertebrates, with special reference to the *fovea centralis* or point of acute vision, I shall first give the processes and methods of preparation which I have used and results obtained, and, second, the position of the *area centralis* as indicated by the retinal arteries. The microscopic descriptions and the relation of the position and shape of the eye and arrangement of the retinal elements to the habits of the animal will follow in a later paper.

[1] I wish to thank Dr. C. F. Hodge for valuable assistance and for his method of injecting the eye-ball, thus preserving it for complete sections. I am also very much indebted to Clark University for valuable aid and for apparatus and materials to further this study.

For microscopical purposes and best results it is necessary to obtain the eye fresh, at least not later than an hour after death, and subject it to the action of certain hardening liquids which will permeate and preserve without causing the retina to swell and become wrinkled. With some animals it is quite easy to preserve the retina without its becoming wrinkled or floated off (fishes, amphibians, reptiles, and some mammals), while with others (most mammals and birds) it is a more difficult task.

In order to prevent this folding and floating off of the retina, the eye is injected under pressure and immersed at the same time in a bath of hardening fluid. It is carried thus on up through the different percentages of alcohol and imbedded in celloidin.

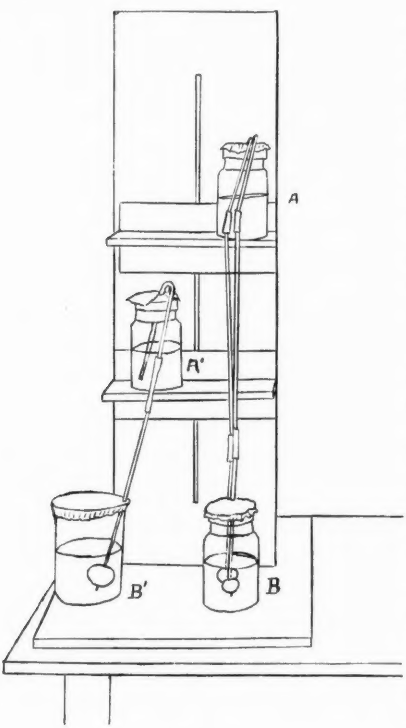

Fig. 1.

A more minute description of the method is as follows: Fig. 1 represents a rack with movable shelves, on which are placed bottles A and A′, containing the same fluid as bottles B and B′, and provided with siphons to connect with glass cannulas.

In order to insert the cannula, a hole is carefully drilled about the equator and on a meridian perpendicular to the plane in which it is desirable to obtain sections. The perforation is stretched open, rather than cut, so the sclerotic will clasp the neck of the cannula tightly. A convenient instrument for this operation is a spear-pointed dissecting needle, and not

too sharp. At the same time reach forward with the point of the needle and pierce the suspensory ligament and iris in order to open the aqueous chamber. In doing this, care is taken not to injure structures in the plane of the desired sections. A cannula of suitable size, being connected with a siphon from A or A′, is filled with the liquid and inserted. The cannula should have a fine smooth point. Great care is taken in inserting it so that the stream of fluid is not directed behind the retina to float it off. A hole is now made in the opposite side of the eye, the aqueous chamber again pierced and all aqueous and vitreous humor allowed to run out. In some animals this humor is very much more gelatinous than in others, and requires much more pressure to remove it. The hole below is then stopped with a small glass plug (Fig. 2, B), and the eye immersed in hardening fluid (Fig. 1, B). The bottles are now covered as tightly as possible with tinfoil to prevent evaporation and entrance of dust particles. The cannula and stopper should fit so tight that there is no leak. In every case the orientation of the eye is marked before it is removed from the head. This is done by sewing a small tag to the outer layers of the sclerotic (Fig. 2, C).

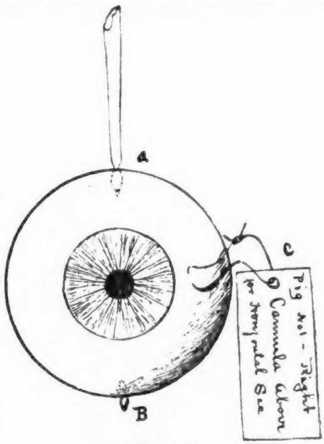

FIG. 2.

The pressure varies greatly with the kind of eye used. Those with thin walls, or containing much cartilage, birds and amphibians, require little pressure, while mammals, in general, can receive much higher. The pressures which I have found to work best vary between 28 and 36 cm.

The hardening fluid used is Perenyi's, in which the eye is allowed to remain twenty-four hours, when it is changed to 70 per cent. alcohol.

In making changes of liquids, great care should be taken that no air get into the eye, and that all the former liquid is

replaced with fresh by removing the stopper in the lower part of the eye. After remaining twenty-four hours in each of the following liquids: 80, 90, 95 per cent., absolute alcohol and absolute ether (1 part each), it is then changed to celloidin. Best results are obtained when three grades of celloidin are used—1st, very dilute; 2d, less dilute; 3d, as thick as will run. It is allowed to remain from four to six days in the first, six to eight days in the second, and ten to fifteen days in the third. If the eye is kept well under pressure throughout this process, the retina will be well preserved and lie smoothly against the choroid.

I have tried other liquids for hardening the eye whole, but with poor success. Have tried the method of Barrett and of Cuccati, but, in each case, the retina was very much wrinkled and folded, while the whole eye was much shrunken and out of shape. In vapors of osmium, I have had fairly good results with the retina, but the same trouble, due to the shrinking of the whole eye, is present. Chievitz says[2] that a fish's eye may be preserved whole, with retina lying nicely back, by simply immersing it, or even the whole head, in 80 per cent. alcohol. The hardening agent which he generally uses is 2.5 per cent. nitric acid.

Another method which I have employed with small animals, especially birds, in order to demonstrate quickly the presence or absence of a fovea, is to immerse the whole head in Perenyi's fluid for from three to five hours. This will harden the eyes so that the cornea, lens and vitreous humor may be removed, leaving the posterior half in situ. With birds I have had good results, the retina lying back smoothly so that the fovea and entrance of the nerve, marked by the pecten, may be easily seen. Fig. 3 represents diagrammatically the appearance of the retina after the front of the eye has been removed.

In order to show the angles which the lines of vision make with the median plane, sections were made through the whole head of several animals (fish, amphibians, reptiles, birds and

[2] J. H. Chievitz, Untersuchungen über die Area centralis retinae. Archiv für Anatomie und Entwickelungsgeschichte, Sup., Band, 1889, p. 141–142.

small mammals), the plane of the section passing through each fovea on the centre of the area centralis. Fig. 4 represents such a section through the foveæ *a* and *b* of a chickadee's head (*Parus atricapillus*), while the lines G H and G I show

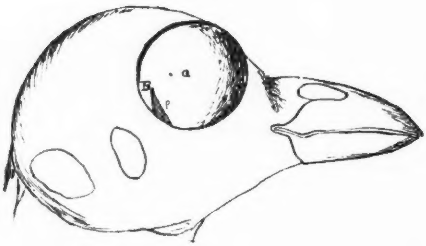

FIG. 3.

Snow-bird (*Junco hyemalis*) x 3.
A, Fovea centralis.
B, Entrance of optic nerve.
P, Pecten.

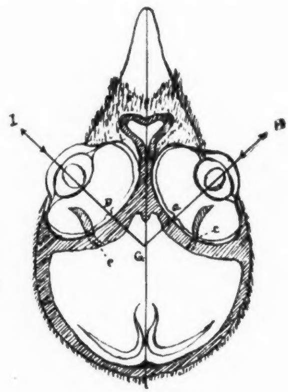

FIG. 4.

Chickadee (*Parus atricapillus*) x 3.
A and B. Foveæ.
C, C, Entrance of optic nerves.
G H and G I, Axes of vision.

the axis of vision. The dotted lines *c* mark the position of the optic nerves which enter in a plane much lower down. In order to harden the whole head, and, at the same time, decalcify the bone, it must remain longer in Perenyi's fluid (about thirty- six hours), and to preserve the cornea and lens in position, a window is made in the top of the eye that the fluids may enter.

Having had good success with simple immersion of the head, this method was tried for hardening the small eyes, and with good success. In fact, the retina proved in good condition, if not better, than when taken through by the injection method. The eye-ball, however, usually caves in when placed in 70 per cent. or 80 per cent. alcohol, but this may be prevented by simply making a small slit through the sclerotic

into the vitreous chamber before immersing in 70 per cent. alcohol to allow the liquids to pass in. Just before putting into celloidin, a window is made parallel to the plane of desired sections, and the hardened vitreous humor is easily removed without injury to the retina or other structures. This method is now used with small eyes instead of the injection, as it is so much easier of manipulation.

In order to show the relation of the retinal arteries to the area and fovea centralis, they were injected with the gelatine-carmine mass of Ranvier. In small animals this injection was made in the carotid arteries, while with large animals the eyes were removed and the injection made into that branch of the ophthalmic artery which supplies the retina. After injection, the eyes were at once cooled and hardened in alcohol. When hardened, the front half of the globe and the vitreous humor were carefully removed, exposing to view the retina, arteries, entrance of nerve, and area and fovea centralis, when present. The fovea is at once seen if it be present, but the area is sometimes very difficult to discern, and, were it not for the blood-vessels acting as land-marks, it might be overlooked altogether. Drawings were made of this posterior half, great care being taken to orient it, so that one would look into it along the axis of vision.

The results of these injections only serve to substantiate Müller's observation.[3] He states that mammals are the only class of vertebrates which possess, in the true sense, a retinal circulation, while with many mammals only a meagre circulation is present (horse and rabbit). Fish and amphibians possess a good circulation in the hyaloid membrane, while birds and many reptiles have the circulation of the pecten. Huschke states that these vessels of the hyaloid membrane and the pecten correspond to the retinal vessels in mammals. They do not, however, penetrate the retina.

With animals which have neither retinal nor hyaloid vessels, it would appear that the retina is nourished by the choroidal vessels. In fact, in animals with good retinal circulation, the capillaries do not penetrate deeper than the outer

[3] H. Müller, Anatomie und Physiologie des Auges, p. 117.

molecular layer, thus leaving the rod and cone, and outer nuclear layers without blood-vessels.[4]

Investigations show that not all vertebrates possess foveæ, but that each class has a representative which does. When there is no fovea, a well-defined area centralis is usually present. However, in some vertebrates, even an area has not been observed.

The following condensed tabulation will show the frequency of the area and fovea centralis in the eyes which have been examined.[5]

Number of different species.		No area found.	Area found.		Fovea.		
			Round.	Band-like.	Simple.	Band like or trough-like	Double.
28	Mammals	13	9	6	2		
80	Birds	1	84	32	85	23	7
9	Reptiles	1	6	2	3	2	
12	Amphibians	3	1	8	1	1	
4	Fishes	3	1		1		

From this tabulation it is readily seen, so far as experiments have gone, that in mammals the presence of a fovea is the exception while an area is the rule. The primates are the only mammals in which a fovea has been found. Most of the mammals examined have a well-defined area which is easily seen, but, in some, an area has not been demonstrated. The arrangement of the retinal vessels, however, indicates the presence of an area which is free from blood-vessels, and may correspond to the area centralis of other animals.

[4] H. Müller, Anatomie und Physiologie des Auges, p. 103.

[5] These results are partly obtained from the tabulation of J. H. Chievitz in his article: Ueber das Vorkommen der Area centralis retinae in den vier höheren Wirbelklassen. Archiv. f. Anat. u. Entwick., 1891, p. 321–325.

With birds, the presence of a fovea seems to be the rule. In fact, the domestic chicken is thus far the only exception. Many birds have a fovea and band-like area, while some have two foveæ and a band-like area connecting them.

In reptiles, the number of species provided with fovea or simple area are more nearly equal, while with amphibians and fishes, the area has frequently not been seen, and the fovea is only seldom observed.

The area centralis varies greatly in form and extent in different animals. It varies from the round form of small extent found in the cat and the weasel to the band-like form found in the horse, sheep, rabbit, frog, etc., which extends horizontally across the retina.

In the case of the fovea we also find a variety of forms and positions. In some animals it is situated on the nasal side of the entrance of the optic nerve (*fovea nasalis*), while in others it is on the temporal side (*fovea temporalis*). According to Müller,[6] in the former case we have monocular vision, while in the latter we have binocular vision. In form it varies from a mere dot-like impression, as in some lizards, to a well marked funnel-like pit in most birds, especially crow, bluejay, robin, etc., and to a trough-like depression in the crocodile which extends horizontally across the retina. Two foveæ have been found in some birds, as in swallows and terns, in which case the fovea nasalis is very near the centre of the retina, and has to do with single vision. It is also larger and deeper than the fovea temporalis, which is situated near the ora serrata and functions in double vision. According to Chievitz,[7] the tern has not only two fovæ, but a trough-like fovea connecting them, and the goose, duck and gull have a round fovea and a band-like area.

A great difference exists in the different vertebrates when their ability for acuteness of sight is considered. It varies from the most perfect sight found in man (and possibly in birds

[6] H. Müller, Ueber das Vorhandsein zweier Fovea in der Netzhaut Vieler Vogelaugen—Zehender, Klinische Monatsblätter, Sept., 1863, p. 438–440; or Anatomie und Physiologie des Auges, p. 139, 142–143.

[7] J. H. Chievitz, Ueber das Vorkommen der Area centralis retinae, Archiv. f. Anat. u. Entwick., 1891, p. 324.

also) where exceedingly fine discriminations are possible, to the limited visual power found in other animals, where only an area centralis is present. Though acute vision and a fovea have always been associated, still we cannot, at present, say that the animals which do not possess a fovea are not able to see acutely. In order to make clear the relation of sight to the habits of the animal, a much more careful observation of its visual habits, and the histological arrangement of the retinal elements will be necessary.

EDITOR'S TABLE.

—THE Antivivisectionists have been endeavoring to get a consensus of opinion on the utility of vivisection, by circulating blanks for signatures, which are attached to a few alternative opinions on the subject in point. The alternatives, excepting those expressing an unconditional affirmative and negative, were not sufficiently precise or well stated to satisfy persons of moderate views, so that it was necessary to amend them more or less to express such opinions. In the summary of the results thus obtained, the antivivisection managers omitted most of these moderate views, and only gave to the public the two extremes. The circulars were also very injudiciously distributed, as a majority of them went to persons unfamiliar with the work of scientific research, as clergymen, etc. The only persons who have a practical knowledge of the subject are original investigators in the natural sciences, physiologists and physicians. The opinions of other persons must be mostly formed at second hand.

As a body of men, those above referred to are at least as humane as any other class in the community. Their business is to relieve suffering, and they are not insensible to those of the lower animals. Naturalists, as a body, are probably more humane in their feelings towards animals than any other class in the community. Nearly all of these men are, however, well convinced not only of the propriety, but of the necessity of vivisection. It is the only method of attacking many difficult problems of physiology. It is the basis of our knowledge of the functions of the human organism, which is itself the first essential to the control of human disease and human suffering. The antivivi-

sectionists are, unwittingly, doing what they can to sustain ignorance and to prevent the relief of human suffering. They are sacrificing their fellow beings, their relatives and their friends, in preference to a few of the lower animals. Men, women and children may suffer and die; white rabbits, guinea-pigs and dogs may live. Such logic is like that of the Spanish Inquisitors, who tortured human beings under the belief that they served God and the cause of religion in so doing. There is, however, less excuse for the antivivisectionists, since knowledge is more widely distributed now than then, and the great utility of vivisection has been demonstrated over and over again.

The six national scientific societies to meet during the holidays in Philadelphia will probably express their views on this subject, and it may be confidently expected that these will accord with those of science the world over.

Intelligent people are best deceived by intelligent frauds. A fraud in order to succeed in the United States must make pretensions to superior knowledge. The alleged or actual graduate of medicine who desires to be a fraud has a pretty good field in this country; and his successes are ever with us, in spite of the opposition of the many true men of that profession. The scientific fraud has not yet developed very largely, as there is no money to be made by pretense in this direction. In fact this species of the genus is not generally a person of evil intentions, and errs chiefly through an active imagination, and perhaps sometimes through a tendency to megalomania.

We are moved to these remarks by reading an article in the December number of a Chicago Journal called Self Culture. On p. 587 we read; "Examination of the brain of such an idiot before its education has begun, shows but few brain cells, and a few nerve fibres connecting them. And when a postmortem has been made upon the child that was once an idiot but that has been lifted up by long years of patient training to citizenship in the moral and rational sphere in which we live and move, such a postmortem shows that an infinite number of brain-cells have been created *de novo;* that fibers becoming necessary have *appeared,* to connect such cells, centers of sensation and emotion and thought."

Now the author of this paragraph should refer us to the published articles which describe the removal of the brains or parts of brains of idiotic children for sectioning and microscopic investigation, and the subsequent replacement of these organs or parts of them in the crania of the children in order that they may undergo the "long years of

patient training" which follow. We would like to know the technique of the operation, and the name of the operator and that of the institution where he operates. Some grown persons might desire to secure his services, and almost everybody could point out some one else, to whom they think such a course of treatment would be useful. Some peculiar conditions might be found which it would be desirable to remove permanently, and so save the "labor of long years" etc.

The editor of the Journal on page 609 stimulates our curiosity further by saying that "Professor Elmer Gates, a psychologist who has for several years been making elaborate studies both in Washington and Philadelphia, has added not a little to our knowledge of the developments of the brain and the relation of particular parts of the brain to thought and emotion and the use of particular parts of the body." The view indeed is not new, but the confirmation given by Prof. Gates researches is very interesting" He then quotes language from Dr. Julius Althaus as to the supposed seat of mental activity in the brain, which embodies a general statement of the little knowledge we have on the subject. The question naturally arises as to the alleged researches of Dr. Gates, and the extent to which they have confirmed our hypotheses on this subject, and if so, as to where they were published? The editor does not tell us. This is a pity, for assertions without authority are useless to science. Is there any connection between these researches and the alleged vivisection of idiots recounted in the article we first quoted? The name signed to the latter is not that of Dr. Gates, so we are quite in the dark. A journal which publishes an article by Sir Wm. Dawson, and writes up the Universities, ought to give us more light no these wonderful researches.

—It is again proposed that the American Association for the Advancement of Science meet in San Francisco in the near future. The Board of Supervisors of that city are said to have extended an invitation to visit the city in 1897. The Association has had many such invitations, and they would have been accepted had the railroad authorities been willing to place their rates within reach of the members. The authorities of San Francisco have, however, this time included in their invitation the British and Australian Associations, and we are informed that the British members will have free or nominal transportation via the Canadian Pacific R. R. It is said that the Dominion of Canada will make an appropriation towards defraying their transportation expenses. Perhaps our Congress would be willing to make an appropriation for securing the transportation of our own members. The amount will not

exceed the outlay on funeral solemnities annually expended by it. Such meetings tend to bring about amicable relations among the living, and to promote the interest in and distribution of knowledge. It might be good politics if the Canadian Boundary and Venezuelan questions should be still on hand in 1897.

RECENT LITERATURE.

Petrology for Students : An Introduction to the Study of Rocks under the Microscope, by Alfred Harker, University Press, Cambridge. MacMillan & Co., New York, 1895. Pp. vi and 306; figs. 75; price $2.00.

This volume of the Cambridge Natural Science Manuals will be heartily welcomed by teachers and students of geology in all English-speaking countries. It presupposes a knowledge of the microscopical features of minerals, and consequently deals only with rocks. These the author divides into Plutonic, Intrusive, Volcanic and Sedimentary rocks. Under each head the general characteristics distinguishing each of the several rock classes are briefly mentioned, and descriptions of the different rock types embraced in each group are given. First come descriptions of the constituents of each rock, then follows a statement of its pecularities of structure. The principal varieties are next mentioned, and abnormal, structural and chemical forms are briefly described. The book concludes with chapters on thermal and dynamic metamorphism and one on the crystalline schists.

Of course, the treatment of the different subjects discussed is necessarily very brief, nevertheless it is full enough in most cases to give the student beginning petrography a very good view of the field. A specially important feature of the work is the large list of references to articles written in English. With this book at hand, students will no longer be required to wait until they have mastered German before beginning the study as heretofore been the case. While by no means exhaustive, the present volume will serve as an excellent introduction to the larger French and German treatises, and will, at the same time, be a good reference book for geologists who do not desire to make a specialty of microscopic lithology.—W. S. B.

Crystallography, a Treatise on the Morphology of Crystals, by N. Story-Maskelyne, Oxford, Clarendon Press, 1895. New

York: MacMillan & Co. Pp. xii and 512; figs. 597, pl. viii; price, $3.50.

This "Crystallography" is a real addition to the literature of the subject that it treats. Its appearance reminds one strongly of Groth's "Physiographische Krystallographie," although the book is by no means a reproduction of the German treatise. The latter discusses the subject from the side of solid symmetry, whereas the former deals with it rather from the analytical point of view. The first 187 pages of the volume treat of the general relations of crystal planes and of zones. The next 200 pages take up the six crystal systems beginning with the cubic, and discuss in order the holosymmetrical and the merosymmetrical forms, combination of forms and twinned forms. Chapter VIII, embracing pages 388–463, is devoted to crystal measurements and calculations, and the final chapter to the projection and drawing of crystals. The plates show the projection of the poles of the most general form and of its derived hemihedral and tetartohedral forms in each system.

It is almost needless to state that the work of the author is based exclusively on the system of indices, known generally as the Miller system. Not only are the faces of crystal forms studied through the aid of the spherical projection, but the individual planes are discussed solely in terms of their normals. No reference is made to other systems of notation, nor to other methods of projection than those elaborated. The book might have been of a little more practical value had the author at least referred to other systems, but its unity might have suffered. As it is, the volume is a very complete exposition of crystallography from the Miller standpoint, and it will, without doubt, prove of inestimable value in popularizing this—the most beautiful method of studying the subject. Of course, the treatment is purely mathematical, but the mathematics used are simple enough to be understood by any one acquainted with the methods of spherical geometry. To the student of minerals too much emphasis will seem to be placed on the theoretical aspect of the development of crystal forms, but to the specialist in crystallography, the emphasis will appear to be placed just where it belongs—on the possibility of deriving all possible symmetrical polyhedrons from certain simple abstract notions concerning pairs of planes, at the basis of which is the principle of the rationality of the indices.

There is no doubt that the treatise before us will appeal less strongly to the student of forms than it will to one of analytical proclivities. Nevertheless it is needed even by the former, if, for no other reason,

because it will impress him more strongly than ever with the exactness with which nature constructs her inorganic structures. With Dr. Williams' little book to develop the imagination of the beginner in crystallography and to interest him in the science, and the present volume to carry him on to a very thorough understanding of the relationships of crystal forms, the English-reading student-world is as well, if not as bountifully, supplied with text books on the subject as are the students of any European country.

The authors discussions are all logically developed, and all his statements are clear and simple. The figures are well drawn and the subjects they illustrate are well selected.—W. S. B.

Elementary Physical Geography, by Ralph S. Tarr. New York: MacMillan & Co., 1895. Pp. xxxi and 488; figs. 267, plates and maps 29; price $1.40.

The most striking features of Prof. Tarr's book are the freshness and wealth of its illustrations and the excellence of its typography. The volume is just what its title indicates, except that perhaps the treatment of its subject matter is a little more inclined toward the side of physiography than toward physical geography. The book is indeed elementary—more so than one would wish, sometimes; at other times it is elementary in the statement of the facts described, while leaving their causes unexplained, where a word or two might have avoided a difficulty which the teacher will surely meet with in discussions with his brightest scholars. In the arrangement of material, some fault can easily be found, but, as the author himself declares, the treatment is, "in many respects, experimental." In spite of these criticisms, the experiment is a success.

The volume is divided into three parts, with four appendices and a very good index. The first part deals with the air. It includes chapters on the earth as a planet, the atmosphere in general, distribution of temperature in the atmosphere, its general circulation, storms, its moisture, weather and climate, and the geographic distribution of plants and animals. Why the first and last chapters included in this part are discussed here is not quite plain. Part second deals with the ocean. It embraces chapters on the ocean in general, waves and currents and tides. Part third treats of the land and its features. A general description of the earth's crust is discussed in the opening chapters. Then follow chapters on denudation, the topographic features of the surface, river valleys, deltas, waterfalls, lakes, etc., glaciers, the coast line, plateaus and mountains, volcanoes, earthquakes, etc.,

man and nature and economic products. The appendices include one on meteorological instruments, methods, etc., one on maps and one containing suggestions to teachers. The last is a list of questions on the text. At the end of each chapter is a list of reference books, with their titles and prices. This is not of much value to the student, but is convenient for the teacher. A list of articles to be found in *Nature*, *Science*, the *Popular Science Monthly*, and similar periodicals might have been of more value in an elementary treatise. However, the plan of referring students to original articles on the subjects discussed is commendable. We can not dismiss the book without another reference to the many really excellent illustrations and charts it contains. The former are, without exception, fresh and new, well chosen to illustrate the author's points and well executed from the bookmaker's standpoint. Many of the charts are original. The volume is, on the whole, the most attractive that we have seen on the subject it treats, and its attractiveness is not at the expense of scientific accuracy. We can safely predict a general adoption of the book as a text in many high schools and academies, and we shall be mistaken if it is not used in some of our colleges, where the instructor desires an *aid* in his work rather than a *substitute* for work.—W. S. B.

Gray's Synoptical Flora of North America.—In 1835 or 1836, Dr. John Torrey planned a Flora of North America, with which Dr. Gray soon became identified, and, in July, 1838, the first part (Ranunculaceæ to Caryophyllaceæ) was published; a little later (October, of the same year), the second part appeared, and in June, 1840, the third and fourth parts were issued, completing Vol. I, the Polypetalæ. As will be remembered, Volume II was not completed, a portion appearing in 1841, and the work being suspended at the end of the Compositæ in 1843 (February). Here the work stopped for many years, and was resumed in 1878 by Dr. Gray (Dr. Torrey having died five years earlier) under the slightly different title of A *Synoptical* Flora of North America. In this volume the Gamopetalæ were completed; in 1884, the Compositæ and preceding families, since whose elaboration more than forty years had passed, were revived. Then shortly afterwards, 1888, came the death of Dr. Gray, followed, in 1892, by the death of Dr. Watson, before the publication of other parts.

In October, 1895, Dr. B. L. Robinson issued the first fascicle of the revision of Vol. I of the Flora, a little more than fifty-seven years since the appearance of the corresponding fascicle. This includes the polypetalous families—Ranunculaceæ to Frankeniaceæ. It includes much

of Dr. Gray's work, to which is added something of Dr. Watson's work, to which we have now added the results of Dr. Robinson's studies.

With such a history, stretching back as it does through more than half a century, it is not to be wondered at that the work is conservative to a marked degree. The sequence of families can differ little from that adopted nearly sixty years ago, and in this fascicle the citation of authorities, the matter of nomenclature, etc., have been made to conform as far as possible to the treatment accorded them seventeen years ago. This extreme conservatism is to be regretted, since science is more productive just as its followers are least tied by the traditions of the past. Yet, with all its conservatism, the Synoptical Flora will be invaluable, and every systematic botanist will hope that health and strength may not fail the present editor before his task is completed.

—CHARLES E. BESSEY.

The Natural History of Plants.[1]—About seven years ago the eminent professor of botany in the University of Vienna, gave to the botanical world a book under the title *Pflanzenleben*, with which botanists soon became familiar as a most useful work. Some time ago the welcome announcement was made that the work was to be translated and brought out simultaneously in England and America. This has now been accomplished, and the result is before us in four good sized volumes, each called a "half-volume," which are attractive externally and internally. On comparing the translation, as brought out by Messrs Holt & Co., with the original, it must be conceded that the former is the by far better done, both in the clearness of text and the perfection with which the printer has brought out the illustrations. The colored plates are especially well done, being printed from the originals by the Bibliographische Institut of Leipzig.

For those who have not seen the original, it may be well to say that it presents in a readable manner (in a *popular* manner, we might say, if the word had not been so dreadfully abused) the main facts as to the structure, biology, and physiology of plants. It is not a text book for daily conning by the student, but it is rather a most interesting work to

[1] *The Natural History of Plants*, their forms, growth, reproduction and distribution, from the German of Anton Kerner von Marilaun, Professor of Botany in the University of Vienna, by F. W. Oliver, M. A., D. Sc. Quain Professor of Botany in University College, London, with the assistance of Marian Busk, B. Sc., and Mary F. Ewart, B. Sc. With about 1000 original woodcut illustrations and 16 plates in colors. New York: Henry Holt & Company, 2 vols., large 8vo. pp. 777 and 983.

be read by not only the botanist, but by every intelligent man and woman who would know something of the deeper problems with which modern botany concerns itself. The topics noted in the table of contents will give some idea of the scope of the work as follows: The study of plants in ancient and modern times; The living principle in plants; Absorption of nutriment; Conduction of food; Formation of organic matter from the absorbed inorganic food; Metabolism and transport of materials; Growth and construction of plants; Plant forms as completed structures; The genesis of plant offspring; The history of species.

A single quotation taken from the opening chapter may serve to show the delightful style in which the work is written: "Some years ago, I rambled over the mountain district of north Italy in the lovely month of May. In a small sequestered valley, the slopes of which were densely clad with mighty oaks and tall shrubs, I found the flora developed in all its beauty. There, in full bloom, was the laburnum and manna-bush, besides broom and sweet-brier, and countless smaller shrubs and grasses. From every bush came the song of the nightingale, and the whole glorious perfection of a southern spring morning filled me with delight. Speaking, as we rested, to my guide, an Italian peasant, I expressed the pleasure I experienced in this wealth of laburnum blossoms and chorus of nightingales. Imagine the rude shock to my feelings on his replying briefly that the reason why the laburnum was so luxuriant was that its foliage were poisonous, and goats did not eat it; and that though no doubt there were plenty of nightingales, there were scarcely any hares left. For him, and, I dare say, for thousands of others, this valley clothed with flowers was nothing more than a pasture ground, and nightingales were merely things to be shot.

"This little occurrence, however, seems to me characteristic of the way in which the great majority of people look upon the world of plants and animals. To their minds, animals are game, trees are timber and firewood, herbs are vegetables (in the limited sense), or, perhaps, medicine or provender for domestic animals, whilst flowers are pretty for decoration. Turn in what direction I would, in every county I travelled for botanical purposes, the questions asked by the inhabitants were always the same. Everywhere I had to explain whether the plants I sought and gathered were poisonous or not; whether they were efficacious as a cure for this or that illness, and by what signs the medicinal or otherwise useful plants were to be recognized and distinguished from the rest."—CHARLES E. BESSEY.

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General Notes.

PETROGRAPHY.[1]

The Origin of Adinoles.—Hutchings[2] has discovered a contact rock at the Whin Sill, England, which, in the author's opinion, represents an intermediate stage in the production of an adinole from a fragmental rock. It contains corroded clastic grains of quartz and feldspar in an isotropic base containing newly crystallized grains of quartz and feldspar. The isotropic material is derived from the clastic grains by the processes of contact metamorphism, whatever they may be, as grains of quartz are often seen with portions of their masses replaced by the substance. The rock has begun its recrystallization from the isotropic material produced by solution or fusion of the original grains, but the process was arrested before the crystallization was completed. The paper concludes with some general remarks on metamorphism. The author thinks that the statement that in granite contacts no transfer of material takes place has not yet been proven true. He also thinks that more care should be taken in ascribing to dynamic metamorphism certain effects that may easily be due to the contact action of unexposed dioritic or granitic masses.

Notes from the Adirondacks.—The limestones, gneisses and igneous instrusives of the Northwestern Adirondack region are well described by Smyth.[3] The intrusions consist of granites, diorites, gabbros and diabases. The gabbro of Pitcairn varies widely in its structure and composition, from a coarse basic or a coarse, almost pure feldspathic rock to a fine grained one with the typical gabbroitic habit.

[1] Edited by Dr. W. S. Bayley, Colby University, Waterville, Me.

[2] Geological Magazine, March and April, 1895.

[3] Bull. Geol. Soc. Amer., Vol. 6, p. 263.

Compact hornblende is noted as an alteration product of its augite. Where in contact with the limestones the gabbro has changed these rocks into masses of green pyroxene, garnet, scapolite and sphene. A second variety of the gabbro is hypersthenic. A third variety is characterized by its large zonal feldspars composed of cores of plagioclase surrounded by microperthite, although crystals of the latter substance alone abound in some sections. The ferromagnesian components are rare as compared with the feldspars. Nearly all specimens of these rocks are schistose, and all of the schistose varieties exhibit the cataclastic structure in perfection. Analysis of the normal (I) and of the microperthitic or acid (II) gabbros yielded :

	SiO_2	Al_2O_3	FeO	MgO	CaO	K_2O	Na_2O	H_2O	Total
I	57.00	16.01	10.30	1.62	6.20	3.53	4.35	.15	= 99.16
II	65.65	16.84	4.01	.13	2.47	5.04	5.27	.30	= 99.71

Near the contact with the limestone the gabbro is finer grained than elsewhere. Pyroxene is in larger grains than in the normal rock, but the feldspar is in smaller ones. The limestone loses its banding and is bleached to a pure white color. Between the two rocks is a fibrous zone of green pyroxene and wollastonite, together with small quantities of sphene and garnet and sometimes scapolite and feldspar. The red gneisses, common to that portion of the region studied which borders on the gabbro, are thought by the author to be largely modified portions of the intrusive rock.

The Eastern Adirondacks have been studied by Kemp.[4] The limestones of Port Henry consist of pure calcite, scattered through which are small scales of graphite, phlogopite and occasionally quartz grains, apatite and coccolite. This is cut by stringers of silicates that are granitic aggregates of plagioclase, quartz, hornblende and a host of other minerals. Ophicalcite masses are also disseminated through the limestones, and these are also penetrated by the silicate stringers. Merrill[5] has shown that the serpentine of the ophicalcite is derived from a colorless pyroxene. The schists associated with the limestones are briefly characterized by the author. At Keene Center a granulite was found on the contact of the ophicalcite with anorthosite.

Hornblende Granite and Limestones of Orange Co., N. Y. —Portions of Mts. Adam and Eve at Warwick, Orange Co., N. Y., are composed of basic hornblende granite that is in contact with the white

[4] Ibid, p. 241.

[5] Cf. American Naturalist, 1895, p. 1005.

limestone whose relations to the blue limestone of the same region have been so much discussed. The granite contains black hornblende, a little biotite, and so much plagioclase that some phases of it might well be called a quartzdiorite. Allanite and fluorite are also present in the rock, the former often quite abundantly. As the granite approaches the limestone it becomes more basic. Malacolite, scapolite and sphene are developed in it in such quantity, that immediately upon the contact the normal components of the granites are completely replaced. On the limestone side of the contact the rock becomes charged with silicates, the most abundant of which are hornblende, phlogopite, light green pyroxenes, sphene, spinel, chondrodite, vesuvianite, etc. The contact effects are similar in character to those between plutonic rocks and limestones elsewhere. The blue and the white limestones are regarded as the same rock, the latter variety being the metamorphosed phase.[6]

An Augengneiss from the Zillerthal.—The change of a granite porphyry into augengneiss is the subject of a recent article by Fütterer.[7] The rocks are from the Zillerthal in the Alps. The gneisses are crushed and shattered by dynamic forces until most of the evidences of their origin have disappeared. The original phenocrysts have been broken and have suffered trituration on their edges, while new feldspar, quartz, malacolite and other minerals have been formed in abundance. The groundmass of the gneiss is a mosaic whose structure is partially clastic through the fracture of the original components and partially crystalline through the production of new substances. The author's study is critical, and, though he treats the described rocks from no new point of view, he discusses them with great thoroughness, calling attention at the same time to the important diagnostic features of dynamically metamorphosed rocks.

Petrographical News.—Ransome[8] has discovered a new mineral, constituting an important component of a schist occurring in the Tiburon Peninsula, Marin Co., Cal. The other components of the schist are pale epidote, actinolite, glaucophane and red garnets. The new mineral, lawsonite, is orthorhombic with an axial ratio .6652 : 1 : .7385, a hardness of 8 and a density 3.084. The axial angle is $2V = 84° 6'$ for sodium light. Its symbol is $H_4 Ca Al_2 Si_2 O_{10}$.

[6] J. F. Kemp and Arthur Hollick: N. Y. Acad. Sci., VII, p. 638.

[7] Neues Jahrb. f. Min., etc., B.B. IX, p. 509.

[8] Bull. Geol. Soc. Amer., Vol. 1, p. 301.

Fuess[9] has perfected an attachment for the microscope which enables an observer to enclose with a diamond scratch any given spot in a thin section, so that it may be easily identified for further study.

Marsters[10] describes two camptonite dykes cutting white crystalline limestones near Danbyborough, Vt. They differ from the typical camptonite in being much more feldspathic than the latter rock. They moreover, contain but one generation of hornblende, corresponding to the second generation in the typical rock, and but few well developed augite phenocrysts, although this mineral is found in two generations.

A portion of Mte. S. Angelo in Lipari consists of a porous yellowish pyroxeneandesite containing grains and partially fused crystals of cordierite, red garnets and dark green spinel.[11]

Cole[12] declares that the "hullite" described by Hardman as an isotropic mineral occurring in the glassy basalts of Co. Antrim, Ireland, is in reality an altered portion of the rock's groundmass, and is no definite mineral substance.

The same author[13] describes the old volcanoes of Tardree in Co. Antrim as having produced rhyolitic lavas instead of trachytic ones as has generally been stated.

GEOLOGY AND PALEONTOLOGY.

On the Species of Hoplophoneus.—Four species of *Hoplophoneus* have already been described; *H. cerebralis* Cope, *H. oreodontis* Cope, *H. primaevus* Leidy and Owen, *H. occidentalis* Leidy. *Dinotomius atrox* will be shown to be a synonym of the latter species. To these may be added *H. robustus* and *H. insolens* herein described. The following key may be valuable in determining the species from a few characters.

A. Skull small, occiput nearly vertical.
    a. Superior sectorial with large anterior basal cusp.
        1. Pms. $\frac{2}{2}$ *H. cerebralis* John Day.
    b. Superior sectorial with incipient anterior basal cusp.

[9] Neues Jahrb. f. Min., etc., 1895, I, p. 280.
[10] Amer. Geol., June, 1295, p. 368.
[11] Bergeat: Neues Jahrb. f. Min., etc., 1895, II, p. 148.
[12] Belfast Nat. Field Club Proceedings, 1894–5.
[13] Geol. Magazine, No. 373, p. 303.

2. Pms. $\frac{3-2}{2}$ pm. 2 reduced or absent *H. oreodontis* White River.
3. Pms. $\frac{3}{2}$ *H. primaevus* White River.

B. Skull large, occiput overhanging.
Superior sectorial with incipient anterior basal cusp.

4. Pms. $\frac{3-2}{2}$ pm. 2 reduced or absent *H. robustus* White River.
5. Pms. $\frac{3}{2}$ *H. insolens* White River.
6. Pms. $\frac{2}{2}$ inferior sectorial with no posterointernal cusp, heel reduced, *H. occidentalis* White River.

Hoplophoneus occidentalis Leidy.

In The Extinct Fauna of Dakota and Nebraska (1869) Leidy described two fragments of a mandible which he thought indicated a species larger than *Hoplophoneus primaevus* and to which he gave the name *H. occidentalis* (*Drepanodon occidentalis*), figuring the specimen in Plate V. No further material was referred to this species until 1894, when Osborn and Wortman in describing a collection of White River fossils in the Bulletin of the American Museum of Natural History determined two specimens as *H. occidentalis*, giving measurements of the more important bones of the skeleton in comparison with those of *H. primaevus*. While pursuing my studies in the American Museum through the kindness of these gentlemen, I found that a complete mandible of specimen No. 1407 from the Oreodon Beds agrees in every particular with Leidy's type, which I have had the privilege of examining in the Philadelphia Academy. A drawing of the mandible accompanied by a faithful copy of Leidy's figure is given in the accompanying plate. Associated with the mandible are several vertebrae and portions of limb bones showing the skeleton to be much larger than the specimen previously determined as *H. occidentalis* in the American Museum Bulletin. They however, agree, as does also the mandible, with *Dinotomius atrox* described by Dr. Williston in the Kansas University Quarterly, January, 1895, from a fine skull and nearly complete skeleton. This specimen which I had the pleasure of seeing last summer I now have no hesitation in referring to *H. occidentalis*. It makes possible the determination of the skeletal characters and affinities, and the restoration promised by Dr. Williston will complete our knowledge of this species. The following measurements are taken from the Kansas University Quarterly.

Length from inion to premaxillary border . . .	260 mm.
Width of zygomata	145 "
Length of mandibular ramus	164 "

Length of humerus	240	"
Width of distal end of humerus	73	"
Length of tibia	237	"
Width of proximal end of tibia	61	"
Width of distal end of tibia	41	"

The relation of *H. occidentalis* to *Eusmilus dakotensis* Hatcher, published in the December NATURALIST, is at once apparent. By comparison with the excellent figure by Mr. Weber, republished by permission in the accompanying plate, it will be seen that *H. occidentalis* stands directly ancestral to *E. dakotensis*, the dentition agreeing very strikingly in the characters emphasized by Mr. Hatcher, but differing in showing an additional incisor and premolar and the presence of a heel on the sectorial. In *Eusmilus bidentatus* Filhol, the type of the genus, the heel is present.

Hoplophoneus insolens sp. nov.

The determination of the characters of *H. occidentalis* makes it obvious that the skeleton determined as such by Osborn and Wortman is a new species. A complete skeleton (number 11,022) and a second specimen with the most of the limb bones and a skull lacking the mandible (number 11,372), both in the Princeton Museum, enable me to determine the skull of this species, a character which is lacking in the American Museum specimen. The particularly close agreement in the size of the skeletons makes either of them typical, consequently I give the measurements already published in the American Museum Bulletin along with measurements from the Princeton specimen as indicative of the size of the species.

The skull of *H. insolens* is long and low, the postorbital constriction very marked, sagittal crest slightly concave, the occiput overhanging and concave from side to side, the posttympanic process is long and massive approaching the postglenoid process and being produced as far inferiorly. The limb bones have stout shafts and relatively small extremities.

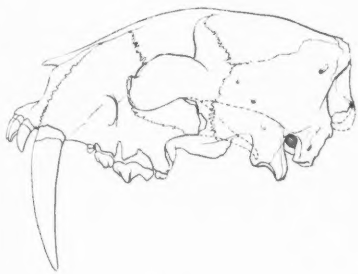
Dentition: I ^{3}, C $\frac{1}{1}$, Pm $\frac{2}{2}$, M $\frac{1}{1}$; the second upper premolar which is variable in the genus usually being absent in this species.

Length of skull, condyles to premaxillary border .	190	mm.
Length of humerus	200	"
Length of ulna	212	"
Length of radius	160	"
Length of femur	250	"
Length of tibia	188	"
Length of pelvis	210	"

PLATE I

3

4



2

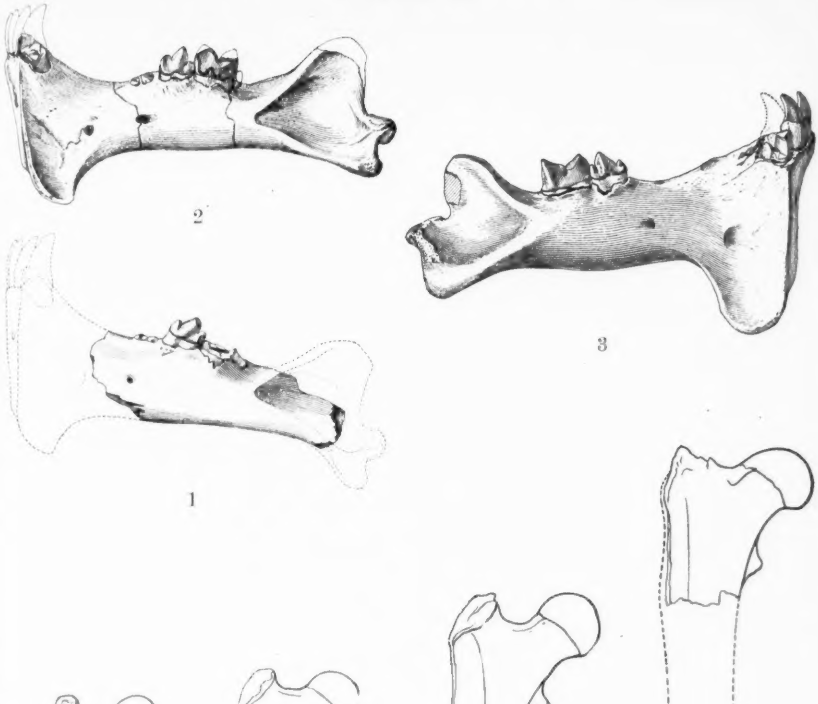
5

1

6

Adams on the Species of Hoplophoneus.

PLATE II.



Adams on the Species of Hoplophoneus.

Hoplophoneus primaevus Leidy and Owen.

The original type of this species is figured in the The Ancient Fauna of Nebraska (1853). Later Leidy figured two skulls in the Extinct Fauna of Dakota and Nebraska, remarking that the larger one might be the skull of an old male, and that the original type was somewhat intermediate in size. The determination of the variation due to sexual characters seems impossible in the case of the extinct cats. However, the material which is available, shows that there are two types represented by these two skulls, the skeletons referable to the types differing more markedly than the skulls. Inasmuch as Leidy's original type agrees more closely with the smaller one, the difference being about such as is presented in any of the species of Machairodonts, it is taken as representative of *H. primaevus.* In the Princeton collection there is a fairly complete skull (number 11,013) and two nearly complete skeletons (numbers 10,741 and 10,934), with the latter skeleton there is also most of the skull. This makes it possible to correlate the skull with the skeletons and give the measurements of the species. The skull is short and high in the frontal region, the orbit horizontally oval, the posttympanic process short, the glenoid drooping considerably below it.

The skeleton is not rugose and the limb bones have slender shafts as in *Dinictis felina.* The dental formula is I $\frac{3}{3}$, C $\frac{1}{1}$, Pm $\frac{3}{2}$, M $\frac{1}{1}$; the second superior premolar probably being constantly present.

Length of skull, condyles to premaxillaries, Leidy's type (approximately)	150 mm.
Length of humerus	160 "
Length of ulna	163 "
Length of radius	122 "
Length of femur	185 "
Length of tibia	143 "

Hoplophoneus robustus sp. nov.

This species is proposed as representative of Leidy's second type or *H. primaevus.* It has its most perfect type in the skeleton and skull (No. 650) determined as *H. primaevus* by Osborn and Wortman, the measurements of which were published in the American Museum Bulletin, Vol. VI, 1894, p. 228, along with those of *H. insolens* (*H. occidentalis*) and which I give here, adding the measurement of the skull. The species is represented in the Princeton collection by specimen

number 10,647, consisting of a fairly well preserved skull and mandible together with a humerus and portions of other limb bones. The skull is relatively large compared with the skeleton. The limb bones are rugose and have stout shafts, being very similar to those of *Dinictis fortis*,[1] and are thus very different from those of *H. primaevus*. Dentition: I $\frac{3}{3}$, C $\frac{1}{1}$, Pm $\frac{2\text{-}3}{2}$, M $\frac{1}{1}$.

Length of skull, condyles to premaxillary border	180 mm.
Length of humerus	170 "
Length of ulna	163 "
Length of radius	132 "
Length of femur	195 "
Length of tibia	160 "
Length of pelvis	180 "

Hoplophoneus oreodontis Cope.

This species is Cope's type of the genus. I introduce it here for the purpose of mentioning a complete skull in the Princeton Museum (number 10,515) which supplements the original type and is, therefore, used here for comparison. The approximate lengths of the femur and tibia are based upon the lengths of these bones associated with the type skull, the epiphyses being lost. Dentition: I $\frac{3}{3}$, C $\frac{1}{1}$ Pm $\frac{2\text{-}3}{2}$, M $\frac{1}{1}$.

Length of skull, condyles to premaxillary border, approximately	135 mm.
Approximate length of femur	120 "
Approximate length of tibia	110 "

Hoplophoneus cerebralis Cope.

This species from the John Day is the smallest of the genus and at the sametime the most peculiar. Cope has pointed out its specific characters as follows: Space for the temporal muscle relatively short; brain capacity large; profile of the face very convex; sagittal crest horizontal; occiput vertical; no paroccipital processes; orbit vertically

[1] In my description of *D. fortis*, American Naturalist, June, 1895, I compared the skeleton with that of *H. occidentalis*, following the description of that species as given by Wortman and Osborn, which the foregoing determination of its skeletal characters shows to be incorrect. *D. bombifrons*, which I described at that time I now find to be a synonym of *D. fortis*; the skull described being correlated with the skeleton and portion of a skull of *D. fortis* by means of specimen number 1400 of the American Museum.

oval. Dentition: I 3/, C 1/, Pm 2/′, M 1/, the third premolar being much reduced.

Length of skull, condyles to premaxillary border (approximately) . . . . 120 mm.

There are thus six species of *Hoplophoneus*, disregarding *H. strigidens* Cope, which being based upon a fragment of a canine exhibiting a peculiar form, is not characterized by any features which refer it to *Hoplophoneus* rather than any other genus. With the exception of *H. cerebralis* they are all from the White River. They present an interesting series both in the size of the skulls and skeletons. The accompanying series of femora give an idea of the relative characters of the skeletons of the larger members of the genus as regards size and strength. Unfortunately nothing is known of the skeleton of *H. cerebralis*, but judging from the size of the skull it would be the smallest of the series, although probably not much smaller than that of *H. oreodontis*. In restoring the femur of *H. occidentalis* I am indebted to Dr. Williston for information as to its length.

The series of skulls figured in outline when taken in connection with the series of femora give an idea of the relative size of the species. The gradation in size is for the most part comparable with the gradation in size of the skeletons. Each species has shown, from careful comparisons and measurements of all the available material, a limited amount of variation, but in no case losing its identity when both the skull and skeleton are taken into consideration.—GEO. I. ADAMS, Fellow of Princeton College.

EXPLANATION OF PLATES.

Plate I.

Fig. 1.—Hoplophoneus cerebralis (after Cope).
Fig. 2.—Hoplophoneus oreodontis (number 10,515 Princeton Museum).
Fig. 3.—Hoplophoneus primaevus (after Leidy).
Fig. 4.—Hoplophoneus robustus (number 650 American Museum).
Fig. 5.—Hoplophoneus insolens (number 11,022 Princeton Museum).
Fig. 6.—Hoplophoneus occidentalis (after Williston).

All $\times \frac{3}{8}$

Plate II.

Fig. 1.—Hoplophoneus occidentalis (Leidy's type).
Fig. 2.—Hoplophoneus occidentalis (number 1,047 American Museum).
Fig. 3.—Eusmilus dakotensis (after Hatcher).

Fig. 4.—Hoplophoneus primaevus.
Fig. 5.—Hoplophoneus robustus.
Fig. 6.—Hoplophoneus insolens.
Fig. 7.—Hoplophoneus occidentalis.

All $\times \frac{3}{8}$

The Goldbearing Quartz of California.—The salient characteristics of the gold quartz veins of California are briefly given by Mr. Waldemar Lindgren in a paper recently published, and the results of his observations are thus summarized :

"The auriferous deposits extend through the state of California from north to south, in an irregular and unbroken line.

"The gold quartz veins occur predominantly in the metamorphic series, while the large granitic areas are nearly barren. The contact of the two formations is not distinguished by rich or frequent deposits."

"The gold quartz veins are fissure veins, largely filled by silica along open spaces, and may dip or strike in any direction.

"The gangue is quartz, with a smaller amount of calcite ; the ores are native gold and small amounts of metallic sulphides. Adjoining the veins, the wallrock is usually altered to carbonates and potassium micas by metasomatic processes.

"The veins are independent of the character of the country rock, and have been filled by ascending thermal waters charged with silica, carbonates and carbon dioxide.

"Most of the veins have been formed subsequent to the granitic intrusions which closed the Mesozoic igneous activity in the Sierra Nevada."

Regarding the origin of the gold, the author speaks with reserve. He points out the possibility of its derivation from the surrounding rocks, which theory, however, is not altogether satisfactory. He then states the following facts and the conclusion based upon them :

"First, the gold quartz veins throughout the state of California are closely connected in extent with the above described metamorphic series and that the large granite areas are almost wholly void of veins, though fissures and fractures are not absent from them.

"Second, that in the metamorphic series the gold quartz veins occur in almost any kind of rock, and that if the country rock exerts an influence on the contents of the veins, it is, at best, very slight.

"Third, that the principal contact of the metamorphic series and the granitic rocks is in no particular way distinguished by rich or frequent deposits.

"It is further apparent that gold deposits have been formed at different periods, though, by far, most abundantly in later Mesozoic times. Some of these later veins may have been locally enriched by passing through earlier impregnations in schist or old concentrations in the sandstones and conglomerates of the metamorphic series, the gold contents of which have, however, only been proved in isolated cases.

"These considerations strengthen the belief that the origin of the gold must be sought below the rocks which now make up the surface of the Sierra Nevada, possibly in granitic masses underlying the metamorphic series." (Bull. Geol. Soc. Am., Vol. 6, 1895.)

Precambrian Sponges.—M. L. Cayeux has published a preliminary note on the spicules of sponges found in the Precambrian beds of Bretagne. The author describes the different forms of the spicules, gives their dimensions, the mode of fossilization, and the probable causes for their fragmentary condition. The principal conclusions derived by M. Cayeux from his researches are (1) numerous spicules of sponges of various species are found in the Precambrian phtanite formations of Bretagne, and (2) that all the orders of sponges with silicious skeletons are represented in these formations.

A resume of the facts ascertained concerning this interesting fauna is given by the author as follows:

"It is impossible not to be struck by the ensemble of the sponges of the phtanites of Lamballe. Even excluding all the spicules which, although they certainly are sponges, yet are too fragmentary for exact identification, there remains an assemblage of forms which points to a very complex fauna.

"In the light of our present knowledge this fauna appears to be composed of Monactinellidæ, probably abundant, Tetractinellidæ, relatively rare, numerous Lithistidæ, and a few Hexactinellidæ. All the orders of Silicea are represented. The branching off of the sponges is then plainly as early as the base of the Precambrian of Bretagne.

"The oldest beds in which any remains have been found belongs to the Archean of Canada. M. G. F. Matthew has described *Cyathospongia? eozoica* from the Lower Laurentian of St. John (New Brunswick) and *Halichondrites graphitiferus* from the Upper Laurentian of the same region.

"*Cyathospongia? eozoica* may be a species of Hexactinellidæ, and *Halichondrites graphitiferus* must be referred either to Monactinellidæ or to Hexactinellidæ. The authenticity of these fossil sponges has been put beyond a doubt by M. Hermann Rauff.

"All the great groups of silicious sponges do not figure in this assemblage, but the fauna presents this character worthy of note, that the Lithistidæ and the Hexactinellidæ, that is to say, the sponges which have the most complex skeleton occupy a prominent place.

"I have called attention to these Cambrian sponges to show that there is no fundamental difference between the Precambrian and the Cambrian sponge fauna. In the one as in the other, we find already traced, the lines along which the future silicious sponges are developed." (Annales Soc. Geol. du Nord T., XXIII, 1895.)

Embryology of Diplograptus.—A large collection of specimens of Graptolites found near Dolgeville, N. Y., furnishes Mr. R. Ruedeman the data for a paper on the mode of growth and development of the genus Diplograptus. The species, *D. pristis* Hall, and *D. pristiniformis* Hall, appear as compound colonial stocks instead of single stipes, as hitherto known. From his observations the writer infers that the colonial stock was carried by a large air bladder, to the underside of which was attached the funicle. The latter was enclosed in the central disc, and this was surrounded by a verticil of vesicles, the gonangia, which produced the siculæ. Below the verticil of gonangia and suspended from the funicle was the tuft of stipes.

It is evident from the structure of these graptolites that the genus Diplograptus has the combined properties of different groups, and gives valuable hints in regard to their common ancestry. The investigation of Mr. Ruedeman is one of the most important recent acquisitions of paleontologic embryology. (Am. Journ. Sci., 1895, p. 453.)

The Upper Miocene of Montredon.—M. Ch. Deperet has just published the results obtained through the excavations he has been making in the hill of Montredon near Bize (Aude), The fossils which he has collected are found also in the peat beds where they are much broken and slightly worn, and in the white marls where he has found more complete specimens, such as skulls and parts of limbs with the bones in proper relation.

Notwithstanding an abundance of fossils, the fauna of Montredon, until now, was characterized by a paucity of species, comprising only Dinotherium, Hipparion, a Rhinoceros and an undetermined Ruminant. The discoveries of M. Deperet have increased the known vertebrates to twelve. There are, in addition to the animals just mentioned, a wild boar, agreeing with *Sus major* of Leberon ; three ruminants, *Tragocerus amalthœus*, *Gazella deperdita*, and Micromeryx ; three carnivores, *Si-*

mocyon diaphorus, Dinocyon, Hyœnarctus arctoides. This last constitutes, says the author, a true intermediate type between Hyænarctus of the Miocene and the bears of the Pliocene, as *Ursus arvernensis* and *Ursus etruscus.* M. Deperet adds that the discovery of this animal fills a gap by revealing in a precise manner the ancestral relation of the bear type. (Revue Scientif., 1895, p. 375.)

BOTANY.[1]

The Vienna Propositions.—(*Continued from page* 1100, *Vol. XXIX.*)—In a succeeding number of the same journal, Dr. Kuntze replies to the foregong article at some length. A considerable portion of the reply is taken up with personalities. This is not without provocation, for Ascherson and Engler have grievously misrepresented him in more than one place in the foregoing article, e. g., in the matter of his proposed 100-year limitation, and his comparison of the changes required by 1737 and 1753—as one can readily see by glancing at Revisio Generum 3[1]. Indeed, they substantially concede the injustice of their accusation as to Kuntze's statement with reference to the changes required by 1753, a few paragraphs beyond, when they discuss their proposed limitation of fifty years. The anonymous correspondent of the *Journal of Botany* who was so pained at the supposed bitterness prevailing in America, is respectfully referred to the pages of the *Oesterreiche Botanische Zeitschrift* for an example of the state of feeling in other lands.

The following extracts will give an idea of Dr. Kuntze's reply.

Of the six propositions of Ascherson and Engler he says: "Numbers 1–4 are not new; No. 5 is a *principium inhonestans,* and No. 6 a supplement to No. 5. The new principle is a year limitation proposal with retroactive force. I had previously proposed a limitation of 100 years only for names sought to be revived in the future, which would only affect old names which are mostly doubtful and undetermined, so that by my proposed limitation, the doubtful cases would be disposed of and greater stability of nomenclature brought about. By the proposition of Messrs. Ascherson and Engler on the other hand, acquired rights would be violated. The gentlemen, indeed, in their last account no longer recognize this right, even as little as the right o political legitimism. These gentlemen now reject also the law o

[1] Edited by Prof. C. E. Bessey, University of Nebraska, Lincoln, Nebraska.

priority, and their proposals have never conformed to the Paris code. One must ask involuntarily what laws Messrs. Ascherson and Engler do recognize in nomenclature at all. With the best intentions, I cannot perceive any trace of a 'Rechtsboden.'"

"The Paris code" he continues, "is in my opinion better than the proposals and deviating principles which Engler, Ascherson and Pfitzer suggest and which they themselves follow only in part. Supposing one followed out the deviating principles honestly and consistently, many more name alterations and complications would result than through following the Paris code."

Since Ascherson and Engler have been at some pains to expose what they deem fundamental errors, one may well suggest a fundamental error upon which they proceed. Their whole argument is based upon the notion that there is a current nomenclature. It is this very notion, indeed, which creates a large part of the opposition to all systematic attempts to bring order into nomenclature. When a systematist goes goes about the work of adjusting the nomenclature of his particular group, current nomenclature does not trouble him at all. There he sets about him with vigor, and even, perhaps, in accordance with rule and principle. But as he looks about him beyond the range of his own group, he feels that it would be very convenient if names could stand as they are in the nearest book at hand, and he becomes conscious of something which he calls current nomenclature. It may be safely affirmed that if Dr. Kuntze had taken up a small group and worked out its nomenclature with the care and thoroughness he bestowed upon all the Phanerogams, no one would have made more than a passing objection, and before long his names would have found themselves current. Who ever said anything about the radical changes made in the nomenclature of the *Uredineæ* when Winter and afterwards Schroeter replaced name after name by the old specific names of *Æcidium* and *Uredo* forms? Very little that Dr. Kuntze has done is more radical than that—and their changes are as current as anything can be said to be at the present day. Before we set about preserving a current nomenclature, we must produce one, and that can only be done by adhering consistently to rules.

As to the propositions made by Escherson and Engler, not much need be said. The 5th and 6th are avowedly only another form of the discredited 4th Berlin thesis. The whole object of the authors seems to be to save their list of eighty-one names—if not by one means then by another. They are as radical as the best of us as far as specific nomenclature is concerned, and one might well suggest that their attitude

towards the eighty-one names they are bent on saving at all hazards, savors quite as much of " legitimism " as anything in the nomenclature controversy. Moreover the propositions are by no means as easy of application as they might appear. The work of restoring prior names has been going on pretty steadily for many years. Since 1891 it has gone on quite rapidly. Are the names restored since the reform movement began to stand, or are we to add a 7th proposition, something like this: " No name recognized since 1891 is to be deemed withdrawn from the operation of the 5th rule ? " Then again it must be decided what shall be considered " use " of a name. If a name appears in a work of wide circulation there is a presumption that it has been used more or less. How many other works must cite it to give it validity ? And must they cite it with approval, or will citation as a synonym or without comment suffice ? What sort of works shall be referred to to ascertain whether a name has been used ? Are names used in catalogues and printed lists used ? If a writer publish two books, say five years apart, and cite his own names, if one of the books comes within the limit, have the names he quoted from himself been used? Or must some other author use them ? The room for individual eccentricity in the application of such a rule is too great to make the rule practicable.

Besides what need is there of pretending to begin the nomenclature of genera with 1753, when in fact it is begun with 1845 ? As Ascherson and Engler point out, their limitation substantially makes it immaterial whether the nominal starting point is 1753 or 1690. The labored distinction between generic and specific nomenclature amounts to very little. It is only partially true that the alteration of a generic name entails the alteration of the name of every species in that genus. Under the Kew Rule it might, perhaps, but otherwise it can scarcely be said that a change of a generic name burdens the memory any more than the change of a specific name. So long as the distinguishing portion of the binominal remains unchanged, each new binominal does not have to be learned over.

In conclusion, without going into the merits of the controversy between Kuntze and Ascherson and Engler, I may say that Dr. Kuntze never hides behind vague general statements, but supports his assertions by citations and actual instances, so that they may be verified Whether one accedes to Kuntze's conclusions or not, he may always know upon what they are based. It would be much easier to determine the value of the assertions made by his opponents if they were in the habit of doing the same. It is easy to declaim against " disagreeable alterations " and to make insinuations as to the motives of the reform-

ers. But the fact remains that Dr. Kuntze has only attempted to do, a little radically perhaps, for all the flowering plants at one stroke, what monographers had been doing piecemeal in every group of the vegetable kingdom. No one objected to their motives, and few to their alterations. Their alterations became a part of "current nomenclature." Had the reform been conducted haphazard and piecemeal, it would have seemed quite proper to many who now vigorously denounce it.—ROSCOE POUND.

The Flora of Ohio.—In the "Catalogue of Ohio Plants" in Vol. VII of the Geology of Ohio, Professor W. A. Kellerman and W. C. Werner make an admirable contribution to our knowledge of the plants of one of the older regions west of the Allegheny Mountains. The catalogue is prefaced by twenty pages or so of historical matter in which we learn that the earliest catalogue of Ohio plants (Miami County) was prepared in 1815 by Dr. Daniel Drake; this was followed in 1818 by a paper on the Scenery, Geology, Mineralogy, Botany, etc., of Belmont County, Ohio, by Caleb Atwater in the *American Journal of Science* (Vol. I), and later, 1831, by Short and Eaton's paper (Southern Ohio) and two by Riddell,—Franklin County, in 1834, and the Flora of the Western States, in 1835, to which a supplement was added in 1836. Then follow lists by Sullivant (1840), Bigelow (1841), Lea (1849), Clark (1852 and 1865), Lapham (1854), Klippart (1858 and 1860), Newberry (1859), Hussey (1872), Beardslee (1874), Wright (1889), besides many short papers in periodicals.

Following the introductory pages one comes at once to the enumeration of plants, in which the arrangement of the families is that of Engler and Prantl, but oddly enough—in *reversed order*. Why the authors gave themselves the trouble to invert the natural sequence is not stated. It is awkward, to say the least. We notice with pleasure that the revised nomenclature has been used, and that all specific names have been decapitalized. Double citations of authorities are given when necessary, and varieties are given as trinomials. Altogether the catalogue is a modern one in plan and execution.

After the Angiosperms, there follow the Gymnosperms, Vascular Cryptogams, Bryophyta, Hepaticæ, Lichenes, Fungi, Algæ and Myxomycetes. Of the last six groups the authors state that the list "must be considered very fragmentary and a mere beginning," yet this is an excellent beginning, of which the State of Ohio needs by no means to be ashamed.—CHARLES E. BESSEY.

The Flora of the Sand Hills of Nebraska.—Mr. P. A. Rydberg has recently published in the Contributions from the U. S. National Herbarium (Vol. III, No. 3) the results of his careful exploration of the Sand Hills of Central Nebraska in the year 1893. Two or three counties in about the center of the sand hill region were selected as the ground to be thoroughly studied, and three months were given to this limited area. Two streams transverse this area, the Middle Loup River and the Dismal River. The former is a rapid stream running down a slope of 8½ to 13 feet to the mile, with hills from 200 to 300 feet high on each side of the rather wide valley (½ to 1½ miles). In its narrower portions the valley is filled with lagoons and swamps, the remains of old river beds. The Dismal River runs through a narrower valley, and the bluffs are higher, ranging from 300 to 600 feet. Away from the rivers Mr. Rydberg found three kinds of sand hills, the first of these are called by him the "barren sand hills," not because they are without vegetation, for they are not, but because they are at present of very little use to man. Here one finds the true Sand Hill vegetation, and when seen from the higher points "the hills appear likes the billows of the ocean."

The Dry Valley Sand Hills constitute the second kind. The hills are long ridges running mostly east and west with long valleys between. The underground drainage is so perfect that little or no water gathers in the valleys, but their rich soil readily yields good crops, or excellent pasturage.

The Wet Valley Sand Hills differ from the last in the greater abruptness of the ridges, which are, in fact, sometimes impassable, and in the less perfect drainage, ponds of water generally occurring at the easterly end of the valleys. In no case is there "surface drainage," every pond being destitute of an outlet. About these ponds grasses grow luxuriantly.

It is evident that the Sand Hill flora is not a homogeneous one. The plants growing along the rivers and about the ponds are very different in character from those which occur on the wooded summits of the "barren sand hills," or the steep slopes of the hills which border the dry and wet valleys. In summing up a discussion of the matter, Mr. Rydberg says: "The most characteristic plants of the sand hills are the four blowout grasses, *Calamovilfa longifolia, Eragrostis tenuis, Redfieldia flexuosa, Muhlenbergia pungens,* of which the first two are found on nearly every sand hill. Next to these the following are the most common or characteristic herbaceous plants:

Andropogon scoparius
Andropogon hallii
Stipa spartea
Stipa comata
Psoralea lanceolata
Psoralea digitata
Carduus plattensis
Opuntia rafinesquii
Euphorbia petaloidia
Euphorbia geyeri
Chrysopsis villosa
Cristatella jamesii
Corispermum hyssopifolium
Croton texensis
Acerates viridiflora
Acerates angustifolia
Acerates lanuginosa
Astragalus ceramicus longifolius
Commelina virginica
Tradescantia virginica
Yucca glauca
Amaranthus torreyi
Frœlichia floridana
Cyperus schweinitzii
Laciniaria squamosa
Cycloloma atriplicifolia
Argemone albiflora

"The most abundant woody plant is *Amorpha canescens,* which is common all over the sand hills. Next comes the Western Sand Cherry (*Prunus besseyi*). On the sand hills around Thedford the third in order is *Ceanothus ovatus. Kuhniastera villosa,* which should, perhaps, be classed among the undershrubs, is as common as any of the class. All these belong to the true sand hill flora. Nearly all the other woody plants are confined to the Middle Loup and Dismal River Valleys. A few, as for instance, *Salix fluvialilis, Symphoricarpus occidentalis, Prunus americana, Amorpha fruticosa* are also found in some of the wet valleys."

The other woody plants along the streams are *Cornus stolonifera, Ribes floridum, Rhus radicans, Rosa fendleri, Rosa arkansana, Ribes aureum, Rhus trilobata, Acer negundo, Fraxinus pennsylvanica, F. pennsylvanica lanceolata, Populus deltoides, Celtis occidentalis, Juniperus virginiana, Parthenocissus quinquefolia, Vitis vulpina, Celastrus scandens, Rubus occidentalis, Ribes gracile, Crataegus coccinea, Ulmus americana* and *Rhus glabra.*—Charles E. Bessey.

Recent Botanical Papers.—Dairy Bacteriology by Professor H. W. Conn comes to us from the U. S. Department of Agriculture, giving the results of the author's work the past three years.—From the same source we have papers on Grass Gardens and Alfalfa, by Jared G. Smith; Fertilization of the Soil as affecting the Orange in Health and Disease, by H. J. Webber; The Grain Smuts, their Cause and Prevention, by Walter T. Swingle; Water as a Factor in the Growth of Plants, by B. T. Galloway and A. F. Woods; Forestry for Farmers, by B. E. Fernow.—From the Proceedings of the Iowa Academy of

Sciences we have Pollination of Cucurbits, Diseases of Plants at Ames in 1894, and Distribution of Some Weeds in the United States, by Professor L. H. Pammel.—Dissemination of Plants chiefly by their Seeds, is the title of a pamphlet of fifteen pages based upon the specimens collected by the lamented young botanist Miss Mary E. Gilbreth, and after her death presented to Radcliffe College. It will prove to be very suggestive to those who wish to prepare similar collections.—" A Guide to find thenames of all wild-growing Trees andShrubsof New England by their Leaves," and " Ferns and Evergreens of New England," are two pamphlets by Edward Knobel, which deserve to be widely used in the public schools. They consist of good figures of the leaves, which should make it possible for even the non-botanical teacher to direct the attention of children to the trees and ferns. They are sold by Bradlee Whidden of Boston for fifty cents each.—We may notice here the beautiful photogravures of fungi issued by C. G. Lloyd, of Cincinnati, Ohio; the last numbers are *Coprinus comatus, Crucibulum vulgare, Lycoperdon separans* and *Urnula craterium.*—Professor T. A. Williams has published (Bulletin 43, Agricultural Experiment Station) a paper upon the Native Trees and Shrubs of South Dakota, in which he lists 37 trees and 80 shrubs. Of these, twelve trees and thirteen shrubs are found in all regions of the State. In the Black Hills, a small region including not more than one-eighth of the whole area of the State, no less than eighty-two of the one hundred and seventeen trees and shrubs are found.—Professor MacDougal writes on Botanic Gardens in the October *Minnesota Magazine.* A half tone illustration of the Botanic Institute at Leipzig, and another of the Botanic Garden at Buitenzorg, Java, accompany the paper.

VEGETABLE PHYSIOLOGY.

Changes Due to an Alpine Climate.—For ten years M. Gaston Bonnier, of Paris, has carried on experiments in various parts of France to determine just what changes occur in plants when they are transported from the lowlands to high elevations. These are described in a bulky paper in *Annales des Sciences Naturelles*: *Botanique*, Sé. VII, T. 20, Nos. 4, 5, 6, entitled Recherches expérimentales sur l'adaptation des plantes au climat alpin. Plants of many genera were removed from the plains, the roots or root-stocks divided into equal parts, and these parts set in similar soil and situations at various elevations, up to several thousand metres, in the Alps and the Pyrenees,

and examined from time to time for anatomical and physiological changes. These soon made their appearance and were as follows, the changes in the plants exposed to the alpine conditions being attributed principally to (1) More intense light; (2) Drier air; (3) A lower temperature. *Change of form and structure:* (1) The subterranean parts as a whole are relatively better developed than the parts above ground. (2) The rhizomes and the roots show little modification, except that the calibre of the vessels is generally smaller and the bark more precocious; (3) The ærial stems are shorter, more hairy, more spread out, closer to the soil and with shorter and less numerous internodes; (4) In general the stems have a cortical tissue that is less thick in proportion to the diameter of the central cylinder; the epidermal cells have thicker walls and the cuticle is more pronounced; often the epidermis is reinforced by a certain number of sub-epidermal layers; the different tissues of the central cylinder are ordinarily less differentiated; when bark exists, it appears earlier and is relatively thicker on branches of the same age; when there are secretory canals, they are relatively, or even absolutely, larger; finally, the stomata are more numerous; (5) Usually the leaves are smaller, except sometimes in sub-alpine regions, more hairy, thicker in proportion to their surface and often absolutely thicker, and deeper green by reflected or transmitted light; (6) The blade of the leaf acquires tissues better suited for assimilation; the palisade tissue is more strongly developed, either by a narrowing and elongation of its cells or by a considerable increase in the number of rows, the cells also contain a greater number of chlorophyll bodies and often each grain of chlorophyll has a greener tint. when there are secretory canals the diameter is relatively or absolutely greater; the epidermis of the leaf shows less differences than that of the stem, nevertheless, in general it is better developed, especially on persistent leaves, which have besides better developed protective sub-epidermal cells; the cells of the epidermis are ordinarily smaller and often the number of stomata per unit of surface is greater, especially on the upper face as M. Wagner was the first to show; (7) The petiole shows modifications generally analogous to those of the stems but much less pronounced; (8) The flowers are relatively much larger and sometimes even absolutely larger; they are more brightly colored and when the color is due to chromoleucites it is the same as in case of the chlorophyll grains, the number in a cell is greater, and often each chromoleucite is of a deeper color; the heightened color occurs also when it is due to substances dissolved in the cell sap. Experiments during eight years with *Teucrium* also show that modifications acquired

by the plant when it is taken from the plain to the mountain, or vice versa, disappear at the end of the same time when the plant is put back into its own climate. *Modification of functions*: (1) If a plant grown on the mountains is transported immediately to the level of one grown on the plains (both originally from the same root) we find for the same surface and under the same conditions, the chlorophyllian assimilation and the chlorovaporisation are more intense in the leaves brought from the alpine region; (2) If the respiration and the transpiration in the dark are compared in the same way, we find that for equal weights these functions have about the same intensity, or are less in the alpine specimens. The paper contains numerous wood cuts showing anatomical details and eleven lithographic plates comparing alpine and lowland individuals of the same species. The last is a double plate in color, illustrating the brighter hues of the mountain flowers. Foot notes refer to the principal literature.—ERWIN F. SMITH.

Spore Formation Controlled by External Conditions.—Einfluss der ausseren Bedingungen auf die Sporenbildung von Thamnidium elegans Link, by Johann Bachmann, is the title of the leading paper in *Botanische Zeitung* for July 16, 1895. *Thamnidium elegans* is a graceful little mould bearing two sorts of sporangia. The sporophore consists of a slender upright stalk, 2–4 cm. high and usually terminated by a single large sporangium, having a columella and bearing many spores. Midway down the sporophore there are usually one to ten or more whorls of branches which ramify dichotomously, often as many as ten times, the terminal divisions bearing singly on their ends small sporangia (sporangiola) generally only 6–8 μ in diameter and containing only a very few spores, usually 1–4. Sometimes only the end sporangium develops and sometimes only the dichotomous sporangioliferous branches; but the cause of this variation which is undoubtedly what led De Bary into the error of supposing Thamnidium a stage in the development of *Mucor*, has remained unknown. By varying his culture media Bachmann has discovered that he can at will produce sporophores with or without end sporangia and with or without sporongiola; in the same way he has been able to change the tiny sporangiola, which frequently bear only a single spore, into big sporangia provided with a columella and bearing many spores. As the result of his experiments he divides the fungus into six types as follows: (1) End sporangium present; sporangiola appearing very early on finely dichotomous branches which may reach the tenth subdivision, spores few. This form occured on more than a dozen differ-

ent media, the best results being obtained from the following: fresh, damp horse dung; dung decoction; agar-agar with 2½ per cent peptone; agar-agar with 4 per cent peptone and 0.5 per cent nitrate of potash. (2) End sporangia present; sporangiola 16–60 μ in diameter, with numerous spores and frequently with a columella and partial swelling up of the membrane. This type was obtained in nine different media, including the following: thoroughly cooked plums; damp bread; eggs; oranges; malt. (3) Only the end sporangium present. Obtained on slightly cooked plums and on 1 volume of malt extract in 2 vol. water. (4) Only the sporangiola present. Obtained in various culture media by raising the temperature to 27–30° C. (5) *a.* Mycelium with thick ends and gemmæ. This form was obtained in the following media: plum decoction with peptone; 1 vol. grape must in 4 vol. water with peptone; 1 vol. malt extract in ½ vol. water. *b.* Mycelium with fine ends and without gemmæ. Obtained in the following fluids: 1 per cent nitrate of potash with 1 per cent Nährlösung; almond oil with Nährlösung; oleic acid with Nährlösung; cane sugar in various percents. (6) Formation of zygospores. Not observed. According to the author, *Th. elegans* is the only fungus known which can be induced to form this or that sporangium, or none at all, by means of purely external, known conditions. He believes the production of the first type is due to substrata in which nitrogenous substances preponderate and fats and carbohydrates are present in only small quantities, and that the second type is due to the reverse of these conditions. The paper contains 24 pages and is illustrated by a double plate.—Erwin F. Smith.

Germination of Refractory Spores.—In spite of every effort, it occasionally happens that the spores of a fungus refuse to germinate either in water or artificial media. This is true of various oospores, teleutospores and ascospores, and particularly and notably of the basidiospores of the whole group of the Gastromycetes, scarcely anything being known of the early stages of species of this group, owing to this fact. Recently, Dr. Jacob Eriksson, of Stockholm, has tried cold on a number of uredospores and æcidiospores with partial success. His method consists in placing the spores for several hours on blocks of ice or in a refrigerator at temperatures ranging down to minus 10° C. In a number of instances spores which refused to germinate in water at room temperatures, either wholly or in great part, did so freely and speedily after being on ice or in a refrigerator. In other cases the cold appeared harmful or without sensible influence, even on the same species. The opinion has been current for a long time that sudden great

changes in temperature favor the development of rust in cereals but usually this has been attributed to the indirect influence of cold in causing a deposit of dew in which the spores could germinate. In the light of these experiments this explanation can hardly be the true one. Spores which refused to germinate after lying in water several days germinated readily after exposure to cold. It would seem as if the cold were capable of stimulating the spores to germinate only when the latter have been rendered receptive by exposure to rainy weather, but further experiments and observations are necessary. It is at least certain that the spores of *Æcidium berberidis*, which germinated badly after cooling, were gathered in dry weather, while those which germinated abnndantly after cooling were gathered (on three different occasions) after several rainy days. The fungi tried by Dr. Eriksson were *Aecidium berberidis, Ae. rhamni, Ae. magelhœnicum, Peridermium strobi, Uredo glumarum, U. alchemillæ, U. graminis* and *U. coronata.* The original paper, entitled Ueber die Förderung der Pilzsporenkeimung durch Kälte, may be consulted in *Centralblatt fur Bakteriologie und Parasitenkunde, Allg.*, Bd. I, p. 557.—ERWIN F. SMITH.

Botany at the British Association.—The presidential address of W. T. Thistleton Dyer before the new Section K (Botany) of the British Association at the Ipswich meeting (*Nature*, Sept. 26, 1895) is an exceedingly well written and interesting paper and one likely to obtain a wide reading. It deals with such topics as the following: Retrospect, Henslow, botanical teaching, museum arrangement, old school of natural history, modern school, nomenclature, publications, paleobotany, vegetable physiology, assimilation, and protoplasmic chemistry. The two and a half columns of sensible remarks on botanical nomenclature are specially commendable to American readers, as also what is said on teaching and in the last three topics of the address. It is certainly a surprise to learn that cramming for examinations from printed texts should be so largely taking the place of the careful study of plant phenomena in many English schools, the tendency in this country of recent years being happily in the other direction.—ERWIN F. SMITH.

Nitrifying Organisms.—Messrs. Burri and Stutzer, of the agricultural experimental station in Bonn, have discovered a bacillus (See *Centrb. f. Bak. u. Par. Allg.*, Bd. I, No. 20–21, 1895) capable of changing nitrites into nitrates and in many respects resembling Winogradsky's organism, but which *grows readily in bouillon and on gelatine.* This bacillus is much larger than the measurements given by Wino-

gradsky; like his it is incapable of converting salts of ammonia into nitrate, but unlike his is motile (when taken from colonies on gelatine or silicates), stains readily, causes slow liquefaction of gelatine, and is not yellowish but varies from colorless to bluish when grown on sillicates. The chemical activity is almost exactly the same as that of Winogradsky's bacillus and these authors, who have been studying the subject for two years, seem to think that it may after all turn out to be the same organism, the differences being less important than would seem at first sight, and resting perhaps on incomplete observations. The most important distinction appears to be the ability of this organism to grow on organic substances, but it does not appear from Winogradsky's publications whether he tried to transfer his organism from sillicate-plate cultures to bouillon, or gelatine, and failed.—Erwin F. Smith.

Relation of Sugars to the Growth of Bacteria.—Unquestionably the most discriminating and important paper that has yet appeared on this subject is a recent one, Ueber die Bedeutung des Zuckers in Kulturmedien für Bakterien (*Centrb. f. Bak. u. Par., Med.*, Bd. XVIII, No. 1), by Dr. Theobald Smith, now of Harvard. Reference is made to the literature of the subject but this is contradictory and many of Dr. Smith's interesting conclusions are largely or wholly the result of his own laborious and brilliant researches. The propositions are stated clearly and it is safe to say that hereafter no one will undertake the study of bacterial fermentation and gas production without first consulting this paper. The author's summary is as follows, but many things are not mentioned in this and the whole paper will repay the careful perusal of all who have groped about in this field of bacteriology: (1) In ordinary meat bouillon, souring and gas formation are only observed when sugar is present. Dextrose is the sugar most commonly attacked and muscle sugar is probably identical with it. (2) The formation of acid results from the breaking up of the sugar; the formation of alkali in the presencê of oxygen results, on the contrary, from the multiplication of the bacteria themselves. So far as tested, the production of acid is common to all anærobic bacteria (facultative or obligate). (3) Facultative anærobiosis is made possible by the presence of sugar. (4) Rauschbrand and tetanus bacilli grew in fermentation tubes only when sugar was present. In test tubes containing the same sugar bouillon multiplication was never seen. (5) As far as tested, all gas-forming species produce along with CO_2 an explosive gas. (6) Souring as well as the production of gas are valuable diagnostic

characters, when at least three sorts of sugar are tested (with exclusion of muscle sugar). (7) Not only must the formation of gas be determined but also the progress of the same, the total quantity, and the quantity of CO_2. (8) For the differentiation of species and varieties it is of value to determine by titration the total amount of acid in 1 per cent sugar bouillon, as well as the germicidal power of such cultures on the bacteria themselves. (9) The division of bacteria into acid and alkali producers must be given up and the conditions govering the production of acid investigated more critically for each species. (10) The existence of fermentable carbohydrates in the digestive tract and in the fluids of the body is probably very favorable to the establishment and multiplication of pathogenic bacteria (both facultative anærobic and obligate, especially the latter).—Erwin F. Smith.

Algal Parasite on Coffee.—Under the title *Cephaleurus coffeæ*, eine neue parasitische Chroolepidee, Dr. F. A. F. C. Went describes in *Centrb. f. Bak. u. Par., Allg.*, Bd. I, No. 18–19, 1895, p. 681, an alga which he has found attacking the Liberian coffee at Kagok-Tegal in Java. This parasite appears on the leaves and berries in the form of round orange-brown spots which look bristly to the naked eye. The alga not only forms a thallus on the surface but sends its threads deep into the intercellular spaces of the host. The presence of the parasite in and on the leaf causes an interesting, protective hypertrophy of the surrounding tissue, the further progress of the alga being soon limited by a dense encircling mass of thick-walled, non-lacunose tissue, developed out of the palisade cells and spongy parenchyma of the leaf. No algal threads were found in this tissue. The berry not being able to defend itself in this way suffers most, becoming gradually brown and finally black and wrinkling and drying prematurely, so that the seed does not ripen. All parts of the alga are subject to the attacks of a fungus, which also appears to be capable of growing in the berries apart from the threads of the alga, but the relation of which to the latter and to the causation of the disease is left by the author in a rather unsatisfactory state. The paper is accompanied by a lithographic plate showing details of the alga and sections of the normal and hypertrophied tissue.—Erwin F. Smith.

ZOOLOGY.

On Bodo urinarius.—Although the discovery of certain peculiar infusoria in human urine dates so far back as 1859, but little is known of these animalculæ. M. Barrois has been investigating the subject

and has recently published his conclusions. According to his account Hassall was the first to detect this microscopic creature in its chosen habitat. He described it under the name of *Bodo urinarius*, as an animalcula $\frac{1}{1800}$ inch long and $\frac{1}{3000}$ inch wide, of rapid motion, generally round or oval, presenting a granular appearance, sometimes they are broader at one end. The long lashes, by means of which they move, are variable in number and proceed when there are two or three to each animalcule from opposite extremities; reproduction by longitudinal fission. In 1885 Kunstler found "small monads . . . . flagellate, transparent and very active . . . . probably *Bodo urinarius*."

In reviewing the subject, M. Barrois gives detailed accounts of these discoveries, and of the condition of the urine in which they appear. He then describes his own methods of investigation, and compares the drawings of specimens, after Hasslar and Kuntsler, with the infusoria he himself had found existing under similar conditions as those described by the authors mentioned. M. Barrois lays particular stress upon the fact that the infusoria found by him, only appeared in urine plainly alkaline, which contained animal matter (broken down epithelial cells, pus, albumen), and which had been exposed sometime to the air. In no case did he find them in fresh urine. Hassall's notes show a similar set of conditions in his case. Kunstler, however, claims to have found the infusoria in fresh urine in company with several species of bacteria. M. Barrois is of the opinion that Kunstler was deceived as to the age of the urine given him for examination, since in all other respects the conditions (as to animal matter, etc.) agree with those of Hassler and the author. In view of these conditions M. Barrois does not agree with the statement made that *Bodo urinarius* is a parasite. He is rather of the opinion that it exists in the air in a spore-like form ready to develop whenever it is brought in contact with a suitable nidus. This it finds in urine conditioned as above described.

In the course of his discussion, M. Barrois refers to *Trichomonas vaginalis* Douné, found by Salisbury in the urine and vaginal mucous of a young girl aged sixteen, supposed to be parasitic, and to certain Trichomonads found by Marchand and also by Miura; in all probability *T. vaginalis*. In the two latter cases, the infusoria was found living in freshly voided urine, so it would appear to be a true parasite. In both cases the urine was loaded with decomposing matter.

By an ingenious experiment, Miura demonstrated that the Trichomonads lived in the urethra only, and was not found in the bladder.

As to the classification of the Monads, M. Barrois considers it extremely unsatisfactory, since it is based on the number and disposition of

the flagella. In fact, *Bodo urinarius*, by reason of its polymorphism, can have no place in such a scheme of classification.

In conclusion, the author compares *Bodo urinarius* with *Oekomonas mutabilis* Saville-Kent, which propagates both from spores and by fission in infusions of vegetable matter, and also with *O. rostratum* Sav.-Kent, found in both fresh and salt water containing vegetable debris. He finds the three species so similar in appearance, that one might infer that their only difference is in their habitat.

M. Barrois repeats, as a final statement, that *Bodo urinarius* Kunstler (=*Cystomonas urinaria* R. Bl.=*Plagiomonas urinaria* M. Braun) can hardly be given a place among the parasites of man. (Revue Biol., Feb., 1895.)

Influence of the Winter 1894-1895 upon the Marine Fauna of the Coast of France.—M. Pierre Fauvel calls attention to the considerable influence which the exceptional lowering of temperature, and long duration of cold, during the last winter, exercised upon the marine fauna of the coasts of France.

Sharp frosts, at the time of high tide, would destroy innumerable quantities of animals that the ebb tide would leave exposed. Annelids, Actinans and Fish were found dead or unconscious, paralyzed by the cold. This mortality, strange to say, extended to depths which the change of temperature could not have affected directly.

Another effect of the cold has been to bring in shore animals ordinarily seen in deeper water, and also certain species very rare or entirely unknown in our fauna. The Spring was marked by an extraordinary abundance of *Balanus porcatus*, which covered with a continuous bed the surface of the boulders and rocks, and by the return of the Mussels which had nearly disappeared. During some weeks *Mytilus edulis* took possession of all the rocks exposed to the southwest wind and formed veritable "moulières" at Dent, Pointe de Réville and at Draguet. Parallel changes are noticed in the annelid fauna. Thus certain species which were common last year have either become rare, or totally extinct, while new species are continually taking their places. (Revue Scientif., 1895, p. 374.)

Preliminary Outline of a New Classification of the Family Muricidæ. By F. C. Baker (Bull. Chicago Acad. Sciences, 1895). On reading this paper we regret to find that Mr. Baker has been putting his new wine into old bottles. In other words, he has borrowed largely from the phraseology of a conchological paper published in 1892, as the following parallel passages show :

PILSBRY, 1892.

"For several years the writer has been accumulating data bearing upon the natural classification of the Helicoid land snails. It has been thought desirable to place before students of this group some of the general results attained, and to invite their friendly criticism.

" * * * the author's aim being simply to place before malacologists the outlines of a classification essentially modern and essentially original."[1]

BAKER, 1895.

"For several years the writer has been accumulating data bearing upon the natural classification of the Gastropod family Muricidæ. It has been thought desirable to place before students some of the results elucidated, and to invite their friendly criticism.

"The author's aim in the present paper has been simply to place before malacologists the outline of a classification essentially modern and essentially original."

[1] The above quotation is from Pilsbry's Preliminary Outline of a New Classification of the Helices, Proc. Acad. Nat. Sci., Phila., 1892, p. 387. Good taste should have forbidden the reproduction by Mr. B. of the second paragraph here quoted, the egotism of which is excusable only in view of its undeniable truth in relation to the 1892 publication. This excuse seems to be lacking in the case of Mr. Baker's paper.

More to the same effect might be quoted, but the above is sufficient on this score.

We do not wish to imply that there is any great harm in using borrowed phrases; they are not copyrighted, and their original author probably does not expect to make use of the same sentences again; but, still, if anybody has ideas worth expression, they surely ought to be worthy of fresh verbiage.

In regard to Baker's subfamilies, we do not see that they differ from those of Tryon and Fischer, except that Baker includes *Coralliophila* and its allies as a third subfamily. As this group lacks teeth, it seems much better to treat it as a family. In this connection it may be well to state that *Latiaxis mawæ* is not a monstrosity as Baker's foot-note (p. 188) would seem to imply.

The diagnoses of subfamilies given are rather absurd in view of their contents, which contradict every word of the descriptions. Not all the genera placed in "*Muricinæ*" have spinous or foliated varices, not all have the nucleus of operculum apical, and not all have few cusps on the rhachidian teeth. What is the use, then, of such a "subfamily?" Among the genera we notice, on a cursory inspection, that *Murex tenuispina* LAMARCK is quoted as type of *Murex* Linné. How can

Lamarck's species, published a half century *later* than Linnæus' genus, be the type of that genus? The type of *Pterorhytis* Conrad ("Pterohytis" Baker) is not *Ocinebra nuttalli* Conr. but *Murex umbrifer*. Other mistakes of this nature occur, but we have not space to notice more.

The citation of the pre-Linnæan "genera" of Klein is contrary to all codes of nomenclature recognized by modern zoologists, and the continuation of such anomalies is to be deprecated. In retaining *Tribulus*, *Pentadactylus*, etc., as of Klein, Mr. Baker is clearly in error.

Most, if not all of the innovations in nomenclature proposed in this paper, are borrowed from Fischer and Dall. We find no new facts in regard to either soft anatomy or shell structure in the entire article, so that Mr. Baker's claims for originalty and modernness do not seem sufficiently apparent to call for special remark.—H. A. PILSBRY.

Herpetology of Angola.—The Herpetology of the Portugese possession in Western Africa, just published by Barboza du Bocage at Lisbon comprises descriptions of 185 species, distributed as follows; Chelonia 10, Loricata 3, Sauria 57, Ophidia 74, Batrachia 41. Of the specimens described, 62 species and varieties belong exclusively to the fauna of Angola and Congo. In order to better appreciate the relation which the herpetological fauna of these two areas bears to that of the rest of Africa, a table of the geographical distribution of the species described is given and forms an important adjunct to the paper. A number of new species are described, and synonymy is corrected. The paper is handsomely illustrated, and forms an important contribution to the knowledge of the subject.

Among the points of interest embraced in the paper are the discovery of the new species: *Naja anchietæ*, *Dendraspis neglectus*, *Vipera heraldica* and *Python anchietæ*; the southern range of the West African *Osteolæmus tetraspes*, *Feylinia currorii*, *Atheri squamigera*, and *Hylambates aubryi*; the northern range of the South African *Maneus macrolepis*, *Zonurus cordylus*, etc. and westward range of the central African *Causus resimus*.

Zoological News. ;BIRDS.—In regard to the question of the value of the forms of the tongues of birds for classification, Mr. F. A. Lucas concludes that in the Woodpeckers the evidence favors the view that the modifications of the tongue are directly related to the character of the food, and are not of value for classification. (Bull. No. 7. Div. Ornith. and Mam. U. S. Dept. Agric., 1895.)

In the study of the hyoid bone of certain parrots, Mr. Mivart finds that the whole order of Psittaci is distinguished from every other order of birds by the shape of its hyoid. The distinctive characters are (1) Basihyal much broadened posteriorly. (2) Basihyal developing on either side a forwardly and upwardly directed process. (3) An *os entoglossum* in the form of a single broad bone with a considerable central foramen, or, in the form of two lateral parts, entoglossals, medianly united in front by cartilage and leaving a vacant space between this and their attachment behind to the basihyal. (Proceeds. Zool. Soc. London, 1895, p. 162.)

MAMMALS.—Mr. Outram Bangs distinguishes the Skunks of eastern North America as follows:

Mephitis mephitica (Shaw), ranging through the Hudsonian and Canadian zones of the east, south to about Massachusetts.

Mephitis mephitica elongata (Bangs), found in Florida and the southern Atlantic states and ranges north to about Connecticut.

Both of these species differ from the western skunks, which form a separate group.

Among the latter the author recognizes Richardson's *Mephitis americana* var. *hudsonica* as a good species which must therefore bear the name *M. hudsonica* (Richardson). It is the largest of all the skunks, and has an extensive range in the northern prairies, extending east as far as Minnesota. (Proceeds. Boston Soc. Nat. Hist., Vol. XXVI.)

ENTOMOLOGY.[1]

Insects in the National Museum.—The staff of the Department of Insects of the U. S. National Museum has been reorganized, as a result of the sad death of the former Honorary Curator, Professor C. V. Riley.

The reorganization has been effected by the appointment of Mr. L. O. Howard, Entomologist of the U. S. Department of Agriculture, to the position of Honorary Curator to the Department of Insects; of Mr. Wm. H. Ashmead to the position of Custodian of Hymenoptera, and Mr. D. W. Coquillett to the position of Custodian of Diptera. All museum custodians are honorary officers. Mr. M. L. Linell will remain as general assistant to the Honorary Curator.

The Department is, at present, in excellent working condition. It contains a very great amount of material in all orders, and, in many

[1] Edited by Clarence M. Weed, New Hampshire College, Durham, N. H.

unusual directions, surpasses any collection in the country. Among others, the following are of especial interest:

(1) The large collection, in all orders, of the late Dr. C. V. Riley.

(2) All of the material gathered during the past 18 years by correspondents, field agents, and the office staff of the Division of Entomology, U. S. Department of Agriculture.

(3) The greater part of the collection of the late Asa Fitch.

(4) The large collection, in all orders, of the late G. W. Belfrage.

(5) The collections in Lepidoptera and Coleoptera made by Dr. John B. Smith down to 1889, together with the types of the Noctuidæ. since described by Dr. Smith.

(6) The collection of the Lepidoptera of the late O. Neske.

(7) The collection of Lepidoptera of G. Beyer.

(8) The collection of Coleoptera of M. L. Linell.

(9) The bulk of the collection, in all orders, of the late H. K. Morrison.

(10) The collection of Diptera of the late Edward Burgess.

(11) The type collection of Syrphidæ made by Dr. S. W. Williston.

(12) The collection of Ixodidæ of the late Dr. George Marx.

(13) The collection of Myriopoda of the late C. H. Bollman.

(14) Sets of the neo-tropical collections of Herbert Smith.

(15) The collection of Hymenoptera of Wm. J. Fox.

(16) The collection of Tineina of Wm. Beutenmuller.

(17) The large Japanese collection, in all orders, of Dr. K. Mitsukuri.

(18) The African collections, in all orders, of Dr. W. S. Abbott, Wm. Astor Chanler, J. F. Brady, the last "Eclipse" expedition to West Africa, and of several missionaries.

(19) The large collection from South California of D. W. Coquillett, in Coleoptera, Hymenoptera, Lepidoptera and Orthoptera.

(20) The Townend Glover manuscripts and plates.

In addition to this material, there are minor collections which have been the result of the work of government expeditions, or are gifts from United States Consuls and many private individuals.

This enormous mass of material is being cared for by the active and honorary forces of the Department, and the perpetuity of the collection is assured. The National Museum building is fireproof, and this, together with the fact that it is a national institution, renders the Department of Insects perhaps the best place in this country for the permanent deposits of types by working specialists in entomology, and for the ultimate resting-place of large collections made by individuals.

The policy of the Museum at large, with regard to the use of its collections by students, is a broad and liberal one. Students are welcome in all departments, and every facility is given to systematists of recognized standing.

On the Girdling of Elm Twigs by the Larvæ of Orgyia leucostigma, and its Results.[2]—The white-marked tussock-moth *Orgyia leucostigma*, has, for a long term of years, been exceedingly destructive to the foliage of the elms, horse-chestnut and fruit trees in Albany. Fruit trees of considerable size have been killed by their defoliation within a few days, toward the maturity of the caterpillar. Large elms and horse-chestnuts have had the foliage entirely consumed, only the ribs and principal veins remaining.

In the summer of 1883, a new form of attack by this insect was observed by me in Albany. About the middle of June of that year, the sidewalks, streets and public parks where the white elm, *Ulmus americana* was growing, were seen to be thickly strewn with the tips of elms two to three inches in length, bearing from four to ten fresh leaves, and comprising nearly all of the new growth of the season. On examination, it was found that above the point where the tips had been broken off, the bark had been removed for an extent averaging about $\frac{1}{10}$ of an inch, apparently by an insect.

As the Orgyia larvæ were then occurring in abundance on the trees, they were suspected as being the authors of this injury, and the suspicion was verified by ascending to a house-top, where the roof was found to be heaped in the corners with the severed tips, and the caterpillars engaged upon the branches in the girdling. The explanation of the breaking-off was simple. With the removal of the bark, the decorticated portion—not exceeding, in many instances, in thickness the diameter of a large pin—dried, and becoming brittle, was readily broken by a moderate swaying of the wind.

The girdling of the twigs in this manner could serve the Orgyia no such purpose as attends the girdling of several other insects, as the *Elaphidion* pruners of oaks and maples, where it enables the insect to attain greater security for its transformations through this method of reaching the ground, or the *Oncideres* twig-girdler, where the dead wood affords suitable food for the larva. Probably the conditions of growth during the spring of this year were such as to render the young bark at the point attacked particularly attractive to the larvæ; but why, after feeding upon it to so limited an extent, it should cease and resume its feeding on the leaves, can not be explained. In a few in-

[2] Read before the American Association for the Advancement of Science, at its Springfield meeting, Sept. 3, 1895.

stances where the twigs had become detached quite near the node marking the commencement of the year's growth, the bark had been irregularly eaten for an inch or more in extent.

While the Orgyia is a serious pest in Albany, it has its years of remarkable abundance and of comparative scarcity. Girdled tips, as above described, have been seen each year since 1883, but by no means corresponding in number to the degree of abundance of the caterpillar. My attention had not been drawn to them the present year, until much later than the usual time—toward the end of August. At this time (21st of August), many tips of unusual length and with perfectly fresh leaves were collected from beneath a large American elm. Each one had broken at the base of the girdling, which had probably been quite near the node of the year's growth. They were of special interest from their great length, varying from 10 to 18 inches. From the growth they had attained, it was evident that the girdling had not been done in the spring or early summer, but in the late summer after the usual brood had completed its transformations. It was clearly the work of a second brood of the insect, and this was confirmed by my having seen, a few days previously from a house-top, while making observations on the elm-leaf beetle, the Orgyia larva about one-half grown.

A distinct second brood of the Orgyia has not been recorded in Albany, although it is known to be double-brooded in Washington and Philadelphia, and probably in Brooklyn, and has also been observed in Boston. The present year, however, has been an exceptional one in the remarkable abundance, the rapid development, and the injuriousness of several of our more common insect pests.

Another interesting feature connected with these tips was the illustration they gave of the manner in which woody structure is built up —the sap ascending through the sap-wood, and, after its assimilation in the leaves, returning through the inner bark and depositing its organized material. The bark above the girdling, in healing in a rough and irregular manner, had swollen out at this point in a bulbous-like enlargement, showing very clearly the arrest and deposit of the returning sap consequent on the absence of its natural channels, and the drying and the death of the decorticated wood below it. In a specimen gathered in which the node of the preceeding year remained attached to the fallen twig, the diameter of the new growth above the bulb was at least twice that of the starved node below.

This peculiar form of Orgyia attack has not been seen upon the horse chestnut, maple, apple or plum, or any of its other food-plants.

J. A. Lintner.

Albany, N. Y.

EMBRYOLOGY.[1]

Experimental Embryology.——Recent numbers of Roux's Archiv für Entwickelungsmechanik contain numerous additions to our knowledge of the possibilities resident in the early stages of the development of animals, possibilities unsuspected till direct experimental interference made them evident.

T. H. Morgan of Bryn Maur presents evidence[2] to show that two blastulæ of the sea urchin, Sphærechinus, may fuse together and form one embryo. When eggs are shaken just after fertilization they may loose their membranes and afterwards some of the resulting blastulæ are found to have twice the normal size though otherwise like the usual blastulæ in appearance. Such large blastulæ are stated to arise from the fusion of two common blastulæ.

Notwithstanding this complete fusion the future development of such enlarged blastulæ gives evidence of their dual origin. At the gastrula stage *two* invaginations are formed.

One may be much the greater and the two may not appear at the same time. The two invaginations stand in no fixed relation to one another and may appear in all parts of the compounded blastula.

Later the larva that develops from two fused blastulæ tends to develop two sets of arms and two systems of skeletal rods, but those accompanying the lesser invagination are much reduced in size and less perfect than the rods associated with the main invagination.

A second paper[3] by the same worker records a variation in the cleavage of the above sea urchin when some of the eggs were shaken.

While most of the eggs divide into 2, 4, 8 and 16 cells some were found to divide at once into *three*. These 3 cells are elongated parallel to the planes that produced them. When they next divide they all do so lengthwise, in flat contradiction to "Hertwig's law." These six equal cells lie in a plane at right angles to the two cleavage planes that have produced them.

Such eggs may develop into gastrulæ. They form six small cells or micromeres at one pole of the mass in place of the normal four. The author thinks "a micromere field must have been present in the egg prior to division."

[1] Edited by E. A. Andrews, Baltimore, Md., to whom abstracts, reviews and preliminary notes may be sent.

[2] Vol. II, pps. 65–70.

[3] Idem, pps. 72–80.

Eggs that have not been shaken sometimes divide at once into four cells.

In both these unusual forms of cleavage the author finds that the three or the four archoplasmic centres present in the egg take unequal numbers of chromosomes. Thus in one case one centre was accompanied by 17, another by 14, another by 33 and the fourth by 0 chromosomes.

That this inequality is greater in the four-fold than in the three-fold division explains, the author suggests, the fact that fewer eggs develop from the four-fold than from the three-fold cleavage.

A third paper[4] gives a detailed account of the partial larvæ obtained when the eggs of Sphærechinus are shaken into fragments. Very minute gastrulæ only $\frac{1}{64}$ part of the volume of a normal gastrula are thought to come from isolated pieces with $\frac{1}{40}$ to $\frac{1}{30}$ the volume of the whole egg.

It is found that the number of cells in such small blastulæ is less than the normal number and roughly proportional to the size of the blastula.

The size of the nuclei, and probably of the cells also, is less in the small blastulæ than in the normal ones.

If one of first two cells of a cleaving egg be isolated it may form a blastula with $\frac{1}{2}$ the normal number of cells. One of the first four cells gives a blastula with $\frac{1}{4}$ the normal number, or with a little more than $\frac{1}{4}$; while one of the first eight cells when isolated produces a blastula with more than $\frac{1}{8}$ the normal number.

Such blastulæ will develop into gastrulæ.

A piece of the wall of a blastula when broken off by shaking may develop into a gastrula.

The little blastulæ formed from fragments of eggs tend to invaginate as many cells as possible up to the normal number for a normal gastrula.

These remarkable numerical relations lead the author to suppose that the reason why isolated cells of later stages in cleavage are not able to develop by themselves lies not in any differentiation of nuclear substance but in the fact that such cells being themselves the results of a series of cleavages cannot produce cells enough for the next stages of development.

Morgan and Driesch publish conjointly[5] their reinvestigation of the remarkable halflarvæ obtained by Chun.

[4] Idem, pps. 81–124.

[5] Idem, pps. 203–226.

Chun's work was done 18 years ago and was, as stated in a letter to Roux ;[6] as follows.

When the first two cells of the eggs of the lobate Ctenophore, *Bolina hydatina* were separated by shaking each developed as a halflarva with four ribs or bands of locomoter appendages instead of the normal eight, two entodermal sacs in place of four and only one tentacle in place of two.

The first cleavage plane coincides with the sagittal plane of the adult and the second with the transverse.

Half-larvæ with 4 ribs, 4 meridional vessels one tentacle and an oblique stomach *may become sexually mature,* developing eggs and sperm under the two subventral meridional vessels!

The missing half is regenerated during the postembryonic metamorphosis.

Driesch and Morgan worked on another Ctenophore, a nontentaculated form, *Beroë ovata* and finding it impossible to employ the shaking method cut the eggs with special scissors.

Isolated cells of the two cell stage develop into blastulæ, gastrulæ and finally into larvæ that are most remarkable in being neither complete nor halflarvæ but larvæ deficient in certain organs.

The cleavage of such an isolated cell is much as it would be if still associated with the other cell in a normal egg : it is a half cleavage as compared with a normal egg. This, however, is not true of the cells that form the ectoderm but only of the peculiar group of cells forming the entoderm. The former cells grow over the half-group of entoderm cells and form a larva that is complete on the surface.

The final larva is abnormal in usually having only 4 ribs instead of 8 and 3 pouches instead of 4.

A second series of experiments seems to throw much light upon the influence of protoplasm versus nucleus in the causation of such imperfect development.

When a piece of the protoplasm of an entire egg is cut off, the egg, deprived of some protoplasm but with it nucleus intact, as far as known, develops into a larva that may be deficient in just the same way as is a larva reared from one of the isolated cells.

In another paper[7] Morgan finds the shaking method will not succeed with the blastulæ of Sphærechinus as they die when shaken. In Echinus, however, both blastulæ and gastrulæ may be shaken into pieces that will live.

[6] Idem, Oct. 25.95, pps. 444–447.

[7] Idem, pps. 257–266.

When pieces of the wall of the Echinus blastula are broken off they may form little blastulæ again and these may gastrulate. When these little blastulæ invaginate they tend to form more entoderm cells, in proportion to the entire number, than is the case in the normal blastula.

In Sphærechinus the normal blastula has about 500 cells and tends to invaginate about 50; in Echinus about the *same fraction of the whole* is invaginated, for of about 1000 cells about 100 go in to form the entoderm.

When young gastrulæ are shaken they may form abnormal larvæ owing, apparently, to changes in the mesoderm inducing abnormal skeletal growths and corresponding abnormal arms.

Pieces shaken out of the wall of a gastrula will not form into a blastula nor into a gastrula.

Likewise the entoderm when shaken out does not develop. Yet a gastrula that has had its entoderm removed by shaking will continue to grow and form a normal skeleton and arms.

A paper[8] on cross fertilization and the fertilization of non-nucleated pieces of eggs also by MORGAN goes over part of the ground of Boveri's remarkable work.

It is shown that small pieces of eggs of *Echinus miliaris* may be fertilized and develop as far as to the 16 cell stage. As the number of chromosomes in such cleaving masses is, in each nucleus, half the normal number it is inferred that such cleaving masses are the results of the entrance of one spermatozoa into a non-nucleated piece of an egg.

In attempting to cross fertilize pieces of eggs of Sphærechinus with sperm of Echinus it was found that the sperm entered the pieces only in few cases; there is the same difficulty in crossing pieces of eggs as in crossing the whole egg with foreign sperm. The reverse is also the case; sperm of Sphærechinus will not readily enter pieces of eggs of Echinus.

It is, therefore not surprising that no larvæ were found that could be traced to non-nucleated pieces of eggs fertilized by a sperm of another species, which is the great desideratum in attempting to repeat Boveri's work.

When the whole eggs of Sphærechinus are fertilized by sperm of Echinus bastards result that are very variable and not all exact middle states between the larvæ of these two species. When the converse cross is attempted the larvæ are "for the most part very abnormal in appearance."

When the eggs of Sphærechinus are crossed with the sperm of Strongylocentratus the larva is very variable and not an intermediate form.

[8] Idem, pps. 268–280.

The converse bastards also show great variation in the skeleton.

BOVERI[9] republishes, is an amplified form with many new illustrations, his remarkable work on the cross-fertilization of enucleated fragments of sea urchin eggs, translated in the AMERICAN NATURALIST March 1, 1893. After considering the opposing results obtained by Morgan and by Seeliger the author still maintains that he has shown that a larva may be obtained from a piece of an egg without any nucleus and sperm of another species and that such a larva has none of the maternal characters but only those of the male parent, thus showing that the nucleus may transmit characters but that egg cytoplasm alone cannot do so.

The evidence for his conclusious is, however, of an inferential nature and a cautious jury may well hesitate before convicting Boveri of having deprived the cytoplasm of its share in the affairs of heredity.

The evidence as he now presents it seems to be about as follows.

1. When at Naples in 1889 he shook a lot of sea urchin eggs, *Echinus microtuberculatus* in a testtube many were broken into pieces of various sizes, with or without nuclei; when this collection was treated with sperm of the same species larvæ of all sizes down to $\frac{1}{20}$ of the normal were found.

2. When pieces that contain no nucleus, as far as could be seen under the microscope, were isolated and fertilized with sperm of the same species they developed into dwarf larvæ.

3. When the normal eggs of *Sphrærchinus granularis* are treated with the sperm of *Echinus microtuberculatus* some few bastards result. In Boveri's original experiment all these bastards were half way between the larvæ of the parent species, both in external form and in the skeleton, which were very different in each pure larvæ form.

4. When the eggs of *E. microtuberculatus* are shaken and treated with sperm of the same species the larvæ present many abnormalities and some may have characters resembling those of another species.

5. When shaken eggs of *S. granularis* are treated with sperm of *E. microtuberculatus* large and small larvæ are formed; some few of the small ones, only a very few, 10 or 12 in all, were entirely of the Echinus or father type.

6. It was observed that the nuclei in any given area of a larva formed from a nonnucleated piece of Echinus egg and the sperm of the same species were, *on the average*, smaller than the nuclei in the corresponding area of a smaller larva formed from a nucleated piece.

7. The above few bastard larvæ of pure Echinus type had, on the average, smaller nuclei than similar larvæ of type intermediate between the two crossed parent.

[9] Idem, Oct. 22, 1895, pps. 394--441.

The author maintains that the few dwarf bastards that were like the male parent came from nonnucleated pieces of Sphærechinus penetrated by a sperm of Echinus. This, however, is an indirect result of all the above facts, It should also be borne in mind that Morgan, as cited above, was not successful in obtaining a cross fertilization of enucleated fragments and that both Morgan and Seeliger[10] find the bastards between Sphærechinus and Echinus so variable that Boveri's experiment in which they appeared as exact intermediate forms seem an exception and hence may be withdrawn from the evidence.

HANS DRIESCH starting from the standpoint that it has been proved that isolated cleavage cells may produce an entire organism seeks to find where the limits of this power appear in the subsequent stages of development.

He cut blastulæ of Sphærechinus into pieces, chopping at random with scissors in a dish full of gastrulæ. When isolated the larger pieces formed gastrulæ or later larvæ, in most cases.

When the gastrulæ of the starfish, *Asterias glacialis* were cut in the same way some lost the inner end of the gastric invagination together with much ectoderm. After healing over the wound the new end of the gastric invagination enlarged and sprouted out the two cœlomic pouches that would have, normally, been formed from the part that was cut away: the power to form cœlemic pouches was thus vicariously assumed by a part that would normally have produced part of the definitive digestive tract. Such larvæ go on to form a normally three chambered digestive tract from what would, normally, have formed but part of the whole.

The author concludes that the powers of the ectoderm or of the ento. derm cells are as yet not restricted as to what organs or parts they may form in their proper germ layer.

When larvæ with the mesenchyme were cut and a piece with only ectodermal cells was isolated in 53 cut of 99 cases no gastrulation took place but only a healing of the wound though life and activity might last for a week.

In a few cases when the digestive tube was removed from such larvæ it did not grow but died after a few days. In 19 cases where the end of the gastric invagination was removed after it had enlarged and sprouted out the cœlemic pouches 17 did not form new cœlemic pouches. In like manner when a cut happened to remove the skeleton of one side it was not formed again.

We thus soon come to a state in which the primitive tendency of cells to replace others in organ formation seems lost.

[10] See AMERICAN NATURALIST, March, 1895.

In all these cases the author will not grant that any *true regeneration* of lost parts takes place.

In the course of his discussion of the influence of yolk upon gastrulation PAUL SAMASSA[11] states that he has performed the following experiments upon frogs' eggs.

The eggs were injured, without breaking the egg membrane, by an induction shock applied from needle points to certain cells or groups of cells in the eight-cell stage of cleavage.

When the four vegetative cells are injured there may result a cell mass lying in two layers upon a mass of yolk and evidently representing, as seen in actions, a normal gastrula minus parts of its ectodermal structures. On the other hand injury to the animal cells results in a mass of cells lying near an inert mass of matter and possibly representing only entodermal cells.

These facts are interpreted as meaning that the animal or the vegetative cells may continue their development for some time independently of life, or at least of the perfect state, of the other cells.

These eggs die without reforming the injured parts by postgeneration.

In an extensive theoretical part of the paper the author places himself in the main, on the side of Hertwig as believing in epigenesis rather than in any form of preformation.

The last paper that we can notice here is that of AMEDEO HERLIZKA[12] who succeeded in tying a thread about the eggs of *Triton cristatus* in such a way as to completely separate the first two cleavage cells.

Hertwig compressed eggs by this method so that they formed hourglass shaped masses a single embryo finally resulted.

But in these experiments where the egg is separated into two distinct halves each develops by itself. From one half of the egg there results an embryo that may live to have a medullary tube and a notochord. This embryo was formed by a process of cleavage like that of an entire egg and not like the cleavage of half an egg.

The author holds that neither Weismann's nor any other ideas of preformation will suffice to explain such phenomena, but that we must accept some form of epigenesis.

He thinks that each of the first two cells is *totipotent* and normally makes half of the embryo when in the normal union with its fellow because of some *inhibition* of its power to produce the whole. In a case of postgeneration we might suppose that this inhibition was removed for a while and then resumed.

[11] Idem, Oct. 22, 1895.

[12] Idem, pps. 352--366.

ANTHROPOLOGY.[1]

Discoveries at Caddington, England, by Mr. Worthington G. Smith.—M. Renach informed the writer at the St. Germain Museum, in 1893, that a hermit was needed in France to live in the Drift Gravel Quarries and pounce upon chipped blades as they were brought to light in the excavations. This was to illustrate the fact that about four-fifths of the alleged paleolithic implements on exhibition in France were either found on the surface and not in place in the gravels, or bought by collectors, professors of geology and curators of museums, as I bought mine from workmen at the gravel pits.

Nevertheless, there is sufficient evidence of a Plistocene blade chipper in western Europe to satisfy the American critic who will take nothing on faith, and the best of this in recent years is embodied in the work of Mr. F. G. Spurrell, who found a stone blade workshop of Plistocene age under drift gravel at Crayford, England, and in the indefatigable explorations of Mr. W. G. Smith at North London (Stoke Newington) and at Caddington, Bedfordshire.

"Man, the Primeval Savage," by Worthington G. Smith, London (Edward Stanford, 27 Cockspur St., Charing Cross, 1894), tells of the striking discoveries made by the latter in some brick-kiln pits on a hill top near Caddington. These cuttings through the drift, discovered by tracing up relic bearing road ballast to its source, and watched for six years, during which time they were often filled with water or abandoned by workmen at critical moments, revealed what Mr. Smith calls a Paleolithic floor or older surface on which rested a stone blade workshop of Plistocene Age. This was covered by a mantle five to ten feet thick, of contorted drift, unfortunately containing no animal remains, that here overspreads the hill, and developed upon examination the following interesting and novel facts:

1. The blade factory was undisturbed, thus presenting an association of artifical objects full of significance and duplicating the results of Mr. Spurrell at Crayford. Other discoverers had found scattered and isolated specimens in the gravel, here the raw material, the blades more or less finished, the chips and the tools lay just as the Post-Glacial workmen had left them.

2. To the envy of the ordinary searcher for isolated objects in the drift, this range of specimens from one place included scrapers worked

[1] The department is edited by Henry C. Mercer, University of Penna., Phila.

on one side, well specialized leaf-shaped blades, either worked all round or sharpened to points, " punches," knife-shaped blades, hammer stones, "anvils," flaked cores and nodules worked in an exceptional way.

3. Discovered blocks of raw material, flint nodules with chalk still adhering to them, showing that the workmen had pulled them out of neighboring flint bearing chalk beds, lay in piles at the site.

4. Several large nodules had been sharpened at one end, leaving the rest of the nodular surface untouched.

5. The hammer stones found were not the numerous oval flint pebbles lying about the site and showing no signs of pounding (though they had been brought to the spot by workmen), but less regular fragments of flint, sometimes knocked into shape and scored with the marks of battering. Sometimes they weighed from five to six pounds.

6. Large flint masses, called by Mr. Smith "anvil stones," were found, showing slight traces of bruising, which, owing to slight doubts of the explorer, were not preserved.

7. The punches discovered were thin, stalactite-shaped nodules, bruised at both ends, weighing sometimes a pound or more, which with "fabricators," pieces of nicked flint used for flaking, in the explorer's opinion, were found mixed with the blade refuse. As opposed to Mr. Smith's view of flaking by means of stone punches and "fabricators," we know that the North American Indians, when working under similar circumstances, used bone, though a relic forger showed the explorer how the Caddington specimens could be accurately reproduced with an iron hammer and a broken gimlet or awl used as a punch.

8. Cores were discovered from which flakes had been worked (a) by careful blows, (b) by smashing with heavy blocks.

9. A beautifully veined pebble, found at the spot, had been brought there as an object of value by the ancient blade workers.

10. Several piles of apparently selected flakes were discovered.

11. A twin flake, held together by a fine, unsplit section, ready to break at a slight jar, was found with the refuse, showing that the workshop site, an area probably covering nearly an acre, had been very gently overspread with the now overlying drift-material, a deposition which had failed to seriously disturb the situation. Mr. Smith, who was present at the brick-pits, at short intervals, for nearly six years, in gathering this remarkable evidence, repeated observations previously made by him at Stoke Newington, Common, London, where, besides duplicates of many of the specimens referred to above, he found two artifically pointed stakes, a scratched log and a chipped blade resting on the scapula of a Mammoth (now on exhibition at the British Mu-

seum). At another place near Caddington, he had found associated with drift blades and in place a horde of two hundred of the bead-like fossils (*Cocinopora globularis*), with holes artificially enlarged, though at none of the sites were drawings on bone, bone needles or lance heads discovered. One of the most interesting features of the work at Caddington consists in what Mr. Smith calls "replacement," a process previously invented by Mr. F. G. Spurrell, and never before, to my knowledge, applied to drift specimens found in situ.

The two thousand two hundred and fifty-nine flakes unearthed at Caddington were grouped according to color on small trays easily shifted from table to table, and a laborious experimental study of them, lasting for three years, demonstrated the interesting fact that many sets of them fitted together, sometimes reconstructing the original nodule on which the blade maker had worked, sometimes hedging about hollows which, on pouring in plaster of Paris, reproduced the form of the resultant and missing blade.

"I examined and re-examined the stones," says Mr. Smith, "almost daily. I looked at them as a relief from other work and at times when I was tired.

"Not only did I keep my selected stones on the tables for this length of time, but I kept a vast number of blocks, rude pieces and flakes, on certain undisturbed grassy places in the brick-fields for the same three years. Whilst working upon my tables, I sometimes suddenly remembered one or more like examples on the grass, and at an early opportunity, fetched them from Caddington. In making up some of the blocks of conjoined flakes, it often happened that one or more interior pieces would be missing. In some cases, these missing pieces were never found, but in other instances, after the lapse of months, or even more than a year, a missing piece would come to light on the paleolithic floor. It is certain that I have not replaced all the flakes in my collection that are capable of replacement—one reason for this is that many flakes are very different in color and markings on one side from what they are on the other, and it is difficult to remember the markings on both sides. Another reason is that the time at my disposal has not been *unlimited*."

All this demonstrates in a manner, as conclusive as it is novel, that the Caddington site is an *undisturbed* workshop, while the analyses of Mr. Smith and the facts described in his work—Man, the Primæval Savage—take precedence over all recent evidence upon the subject, and throw a new light upon the more ancient subdivision of the Stone Age in Europe.

He who has spent earnest hours upon the problems of Plistocene humanity would gladly have seen a department of a museum specially devoted to these unique discoveries and demonstrations, but in a visit to Caddington in 1894, I learned with regret that the series, highly important from its entirety, and not jealously guarded as a whole, had been dissipated for the sake of collectors who wished to illustrate certain phases of Paleolithic blade manufacture with "fine specimens."

Theory, and with it the desire to propound formulæ for the blade-making process in general, yield respectfully to these toilsome investigations and to the persistent ransacking of quarries by a faithful observer whose work alone answers many of the doubts of the American student, and counteracts the questionable impression left upon the mind of the visitors to European museums by rows of typical specimens bought from workmen or gathered upon the surface.

H. C. MERCER.

Recent Explorations of Captain Theobert Maler in Yucatan.—[Extract from a letter received by the editor, December 9th, 1895].—After your departure from Yucatan, I undertook an expedition to the *Peten'Itza* region (Guatemala), crossing the entire pensinula, whose interior or southern part is nearly unknown.

After examining the country around the great Laguna of *Peten'Itza*, I embarked on a small canoe on the Rio Dela Pasión ("which, farther down, is named *Usumutsintla* [Land of Apes, *Usumatli* = with reverence, *Usumatsin* = Ape; *tla* = there is, there are, place of]). Arriving, finally, after many difficulties at Tenosique (State of Tabasco), from whence the traveler finds at his disposition small steamers plying to Laguna del Carmen, and thence by sea to Progresso. On this journey I had the luck to discover and photograph several highly interesting and unknown cities, with remarkable monuments and splendid sculptures, some in the neighborhood of Laguna del Peten, others on the right and left shores of the Rio Pasión (*Usumatsintla*).

On my return to Ticul, I found your letters and also one from Mr. Ashmead, which latter I answered, referring him on the subject of aboriginal Syphilis and Lupus to some passages in the ancient Spanish authors.

As to pottery-making, I have observed that it is the work of women solely, who exercise the art, in my opinion, in the ancient manner serving themselves nearly exclusively with the hands and feet and without special instruments. Here at Ticul, it is easy to see them at work, as the industry is a common one in the suburbs.

My collection of ancient earthen vessels is quite interesting, but as you left Ticul in such a hurry I could not show them to you. Several

of my vases have quadrangular inscriptions, of which I have not yet had time to make photographs. Lately the Globus published accounts of several of my smaller expeditions, accompanied by some twenty photographical illustrations which you may perhaps see in the Globus, Nos. 16 and 18, for 1895.

Some days ago, an earthen vessel, full of little implements of worked stone, was found at a hacienda near Ticul. I have been promised the specimens, and will communicate with you in case they turn out to be of interest. From the cave of *Loltun,* I have several very good photographs *Lol* = *Bejuco,* the Haytian name for hanging plants (the name *Vana* is not used in Mexico); *tun* = stone; *Loltun* = stalactites = hanging stones or stones like hanging plants.

I shall be glad to publish, from time to time, in American scientific or popular journals, small articles describing my Yucateckan discoveries, and when my present work of enlarging photographic negatives is finished, shall be ready to prepare for you a series of accounts of my work, accompanied by the necessary celluloid positives from which it is easy to make reversed negatives for the photolithographic process.

Next year I shall return to the States of Tabasco and Chiapas, where I have still to explore several entirely unknown ruins hidden in the wilderness occupied by the *Lacandones* Indians.

—Theobert Maler.

Ticul, November 20, 1895.

SCIENTIFIC NEWS.

The Biological Station of the University of Illinois is first to issue its circular for the summer of 1896. The station staff is composed of Professor S. A. Forbes, Director; Dr. C. A. Kofoid, Superintendent; Frank Smith and Adolph Hempell, Zoological Assistants; Dr. A. W. Palmer and C. V. Millar, Chemists; C. A. Hart, Entomologist and B. M. Duggar, Botanist. The station is situated upon the Illinois River near Havana, Ill., and is equipped with every facility for collection and study. There is a floating laboratory sixty feet long and twenty wide, a steam launch, licensed to carry 17 persons, and all the necessary supplies of tables, microscopes, aquaria, nets, chemicals, etc., as well as a specially selected library. As there are accommodations for only 16 in addition to the station staff, applications for the coming summer will be received only from those who have had sufficient experience to place them beyond the need of continuous supervision in their investigations, and, other things being equal, instructors in biology in colleges and high schools will receive the preference. The station will be open

during June, July and August. An incidental fee of $5.00 a month will be charged, and no application for tables should be made for less than two weeks. Board and rooms can be had in Havana at from $4.00 to $5.00 a week. All applications should be addressed to the Director, Professor S. A. Forbes, Urbana, Ill.

The announcement is made that Professor Marshall Ward has been elected to the Chair of Botany in the University of Cambridge, England, to fill the vacancy occasioned by the death (July 22, 1895) of the venerable Professor C. C. Babington.

The University of Cambridge receives the botanical collection of the late Professor Babington.

Mr. F. B. Stead, of Cambridge, England, has been appointed to carry on the investigations of the fisheries at the Plymouth Laboratory, and Mr. T. V. Hodgson as Director's Assistant in the same institution.

After an interregnum of several years, Washburn University, Topeka, Kan., has appointed Dr. G. P. Grimsby, of Columbus, Ohio, to the Chair of Geology and Natural History.

Drs. Walter B. Rankin and C. F. W. McClure, of Princeton, have been advanced to Professorships in Biology in the College of New Jersey.

The Government of the Cape of Good Hope has recently established a geological commission to carry on a survey of that region.

Dr. R. H. True has been appointed Instructor in Pharmacognostical Botany in the University of Wisconsin.

Dr. W. S. Strong, of Colorado, is called to the Chair of Geology in Bates College, Lewiston, Maine.

Bernard H. Woodward has been appointed Curator of the Museum at Perth, W. Australia.

Dr. R. Metzner has been elected Professor of Physiology in the University of Barcelona.

Dr. Dalle-Torre is now Assistant Professor of Zoology in the University of Innsbruck.

Dr. Hans Lenk has been appointed Professor of Geology in Erlangen.

Dr. Ducleaux has been elected President of the Pasteur Institute.